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Political Science Quarterly

Edited for the
Academy of Political Science
by the Faculty of Political Science
of Columbia University

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Toward A Unified Social Science
CARL LANDAUER

Local Leadership and the Typology of New England Towns, 1700-1785
EDWARD M. COOK JR.

Labor Forces and Race Relations: A Comparative View of the Colonization of Brazil and Virginia
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Giolitti's Reform Program: An Exercise in Equilibrium Politics
SÁNDOR AGÓCS

Reviews

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Contributors to the December Issue

CARL LANDAUER is professor emeritus of economics at the University of California at Berkeley. Most of his major publications are in the field of economic systems and in the history of social movements.

EDWARD M. COOK JR. is an instructor in the department of history at the University of Chicago and a specialist in American colonial history.

RICHARD R. BEEMAN is assistant professor of history at the University of Pennsylvania. He is currently at work on a book on the political structure of late eighteenth-century Virginia.

SÁNDOR AGÓCS is assistant professor of history at Wayne State University. A specialist in modern European intellectual history, he is at present working on a comparative study of German and Italian labor ideologies and organizations at the turn of the century.

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Toward A Unified Social Science

CARL LANDAUER
University of California, Berkeley

There is no important fact of economic life which does not have political significance and which does not influence the general structure of society; there is hardly any event in the sphere of government which does not affect the production of goods and services or the distribution of income, or both, and which does not cause changes in many interpersonal relations; there is no change in such institutions as the family or in such habits of thinking as the prestige accorded to some ethnic components of society which has no bearing on the economy or on politics. The interrelationships which these examples illustrate have never been denied by any social scientist; they constitute a strong case for an interdisciplinary approach to social problems. Such an approach has been attempted. A good many institutions of higher learning have courses in "integrated social science"; there are textbooks for these courses; there is also a body of literature which, without an immediate instructional purpose, tries to lay the groundwork for an integrated approach. Yet the success of these efforts has been meager. Few economists pay more than lip-service to the recognition of the general societal framework within which economic operations take place; the disregard for basic economic facts by many sociologists is a patent evil; political scientists, while they have taken cognizance—at least in a rudi-

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mentary fashion—of some basic facts of economic life and have even engaged in efforts to use decision and organization theory as developed mainly by economists and social psychologists, have done little to get economists and sociologists interested in the political significance of the latter's own findings.

This relative ineffectiveness of the integration efforts has resulted in part from the difficulty of the task and in part from the manner in which the efforts have been undertaken. In any field of knowledge, an attempt to synthesize has to struggle against the tendency to specialize. This latter tendency has brought tremendous rewards. We cannot abandon specialization but we can complement it by looking across the borders of individual disciplines and by curbing the esoteric character of the techniques employed by the individual disciplines wherever that is possible without endangering the effectiveness of the scholarly effort.[1]

In some instances specialization has even aided the efforts to combine the work of social scientists from different fields. This has occurred when the object of specialization was an empirical problem at the crossroads of various disciplines. When such an object was selected, say, by an economist, who found that the concepts and techniques of his own discipline would not enable him to arrive at valid conclusions, he often formed a team with scholars from neighboring fields. In this manner our knowledge has been enriched by interdisciplinary studies in such areas as societal development and urbanistics. But these studies have produced little progress toward the solution of the core problem in welding the social sciences. This task consists of considering and analyzing the central concepts of one discipline, and the underlying phenomena, from the viewpoints of the others—to show,

[1] The tendency toward specialization inherent in modern research methods has been aggravated by institutional arrangements which, in the academic world, give the specialist a career advantage over the generalist. This is an interesting example of the interrelationship between epistemology and the way social institutions operate. In their tendency to specialize, the social sciences have emulated the natural sciences. In a sense, however, the tendency has been pushed farther in the former fields than in the latter. There is no biologist who does not have at least a nodding acquaintance with chemistry and physics; the same goes for an astronomer, and, of course, every physicist has to know the fundamentals (and often much more) of chemistry and vice versa. Nothing like this is true among economists, sociologists, and political scientists.

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for example, the political and sociological significance of the economic market, the economic and political significance of changes in mores, the effects of political centralization and decentralization on nonpolitical human relations. Why have the efforts to achieve this kind of integration been sporadic rather than systematic and their results therefore very limited?

I

If we survey the literature on integrated social science—leaving aside the empirical studies of particular phenomena—we find that it falls into two categories, either of which suffers from a special weakness. Textbooks consist in most instances of a part written by a sociologist, another part by an economist, still another by a political scientist, each author explaining the fundamentals of his own discipline. Sometimes a social psychologist, a historian, or a philosopher is added to the group or substituted for the representative of another field. Such juxtaposition is not integration. At best, it gives the student of one of the social sciences a smattering of the concepts and findings of the others. But the student cannot for himself do what the authors fail even to attempt—merge these various viewpoints into a unified approach.

The second category of books undertakes to effect such a merger. These books are invariably of high scholarly caliber, but have been almost as ineffectual as those of the first category in producing a mutual permeation of the approaches of the individual disciplines. To achieve their purpose, these books develop a huge superstructure of new concepts into each of which economic, political, or other societal phenomena are to be fitted. The scholar who wants to use this apparatus in order to describe the relationships among the objects of the various disciplines must learn to understand completely the meaning which the authors connect with these concepts.

The use of some common concepts for all the disciplines is indeed indispensable for their integration. Later this essay, for instance, will employ the term *social mechanism* to cover both the economic market and the rule of law, because what they have in common must be recognized in order to understand some important societal phenomena. But if the common concepts reach the degree

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of complexity which they do in practically all the books on integrated social science that aim at true integration instead of mere juxtaposition, they miss their purpose, because learning this new language detracts too much energy from the recognition of substantive relationships.

To appreciate the importance of this point, we have to realize that the drifting apart of the social sciences since the days of Max Weber, Joseph Schumpeter, and J. R. Commons has been to a great extent due to the development of specialized techniques and specialized hierarchies of concepts. Economics, for instance, has always considered its special task to explore the implications of rationality with regard to the utilization of material resources and—since rationality presupposes goals and therefore human motives and since no conclusion is possible if every conceivable motive is admitted—has always operated on very simplified assumptions with regard to motivation. But until economists generally—not only in the exploration of problems especially suitable for algebraic or geometrical analysis—came to use mathematical language, a great deal of communication took place between economists and sociologists with the result that the latter could help the former in correcting the results reached on the basis of oversimplification, by taking cognizance of a greater variety of human motives. Today most economists, if they are at all interested in the general problems of society outside the use of material resources, pursue this interest as citizens without any significant connection with their thoughts as professional men; figures, like Kenneth Boulding, who try to connect their role as economists with thoughts about political and sociological problems are entirely exceptional. On the other hand, the sociologists, in trying to develop a schematization of interpersonal relationships and of the formation and operation of institutions, have built up a voluminous body of concepts, which are expressed in a vocabulary that puzzles and bewilders all outsiders. As a result, communication with other social sciences has largely ceased. If a similarly complex body of concepts is built up to accommodate what is common to all the social sciences, integration will become another specialty; it will not be a work-a-day habit of any substantial number of economists, sociologists, and political scientists.

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We can illustrate the point by reference to one of the outstanding books of the second category, Talcott Parsons and Neil Smelser's *Economy and Society*.[2] In so doing, we have to keep in mind that the question is not whether the book makes a valuable contribution to our knowledge but whether it supplies a generally usable set of tools for the integration of the social sciences. The authors of this book regard society as a system and the economy as one of its subsystems. They use the term "system" in the sense in which it is generally employed by sociologists: a set of elements of an operational entity which depend for their functioning on each other—among which there is, therefore, interaction.[3] A "social system," according to Parsons and Smelser, "is the system generated by any process of interaction, on the socio-cultural level, between two or more 'actors.' The actor is either a concrete human individual . . . or a collectivity of which a plurality of persons are members."[4]

The authors believe that the "fundamental imperatives" to which any social system is subject can be divided into four categories: "pattern maintenance (including tension management), goal attainment, adaptation and integration."[5] And "The goal of the economy . . . is to produce generalized facilities, as means to an indefinite number of possible uses. The important feature of this production is not only the quantity of such facilities . . . but their generability, i.e. their adaptability to these various uses."[6] Another subsystem of society is "polity," which includes but is not coextensive with government: "the goal of the polity is to maximize the capacity of the society to attain its system goals, i.e. collective goals. We define this capacity as power as distinguished from wealth."[7] The specific function of the polity

[2] Talcott Parsons and Neil Smelser, *Economy and Society* (Glencoe, Ill., 1956).

[3] In everyday language, we use the term either as essentially synonymous with *method*—for example, "I have a system to win every card game"—or for "a set or arrangement of things so related or connected as to form a unity or organic whole," in which sense we speak of the Bell Telephone System or of our body as a system; the latter is the sociologists' usage.

[4] Parsons and Smelser, 8.

[5] *Ibid.*, 18.

[6] *Ibid.*, 47

[7] *Ibid.*, 48.

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is therefore "goal attainment." A third subsystem is called the "integrative" one, of which the decisive force is "solidarity" or "cohesion"; this is the "generalized capacity of agencies in the society to 'bring into line' the behavior of the system units in accordance with the integrative needs of the system, to check or reverse disruptive tendencies to deviant behavior, and to promote the conditions of harmonious cooperation."[8] The fourth subsystem,

> [the] pattern-maintenance and tension-management subsystem . . . focuses on the institutionalized culture, which in turn centers on patterns of value orientations. Such patterns are relevant to all social action. For any system, however, they are most nearly constant and relatively independent of the urgency of immediate goal needs and the exigencies of adaptive and integrative problems imposed on the system. . . . Pattern-maintenance and tension-management differ from the integrative problem in the sense that they focus on the unit of the system, not the system itself. Integration is the problem of interunit relationships, pattern-maintenance of intraunit states and processes.[9]

Among these subsystems there is exchange. The economy, the primarily adaptive subsystem, receives from the households, which form largely the pattern-maintenance subsystem, its supply of labor, and it delivers income to the households. The economy receives from the polity the physical facilities for the exercise of power.

Since Parsons and Smelser were primarily interested in the relationships between the economy and the other subsystems of society, the relationships among these other subsystems themselves are not described in detail; but the extreme complexity of these relationships becomes clear. The complexity of relationships among the various areas of societal life is, of course, a feature of reality, and the authors merely attempt to adjust their mode of presentation to this feature; but the question arises whether this mode of presentation is optimal for giving the reader a view of these complex relationships—whether the factual complexity is not aggravated for the analyst by the character of the conceptual tools, which makes their application unnecessarily difficult and

[8] *Ibid.*, 49.
[9] *Ibid.*, 50.

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not correspondingly fruitful.

Society can indeed be viewed as a system divided into subsystems. Such a view, however, is an intellectual construct, a method to make reality intelligible, not a part of this reality which we can observe directly as we can observe money, the family, the police, elections, and so on. Because it is a method, it is subject to judgments of methodological expediency. One argument against its expediency is the difficulty of handling the conceptual apparatus; another one, closely connected with it, is the inevitable incompleteness in schematizing the "exchanges" among the subsystems. What we call the economy has certainly as one of its functions, and in some sense as its primary function, the task of supplying individuals, governments, and other entities with the material means to achieve their purposes; it is equally true that many of our goals are set for us by governments and other collective bodies; families and other social structures contribute to the maintenance of value patterns, and no society can live without some such maintenance. Whether an "integrative" function exists independently of this pattern-maintenance may be doubtful. But how much of reality do we really grasp by considering society as an aggregate of subsystems, each characterized by a primary function, which stand in exchange relations to each other?

When we say that it is the primary function of the economy to supply us with material means to fulfill our purposes, this is correct only in the sense that without material means no human purposes can be fulfilled, that no material means can be produced without the activities we call economic, and that the production of material means directly or indirectly[10] induces more persons more frequently to economic activity than any other purpose. But it so happens that these other purposes are of particular significance for the interaction of several social science disciplines, a fact which is obscured by focusing on the function of the economy to

[10] A worker in an automobile factory does his work not in order to produce automobiles but to earn a wage; and the managers of the company which employs him also are usually not interested in production per se but in making a profit or in some other purpose, such as securing prestige or power for themselves, for which the production of cars is a means. In the last instance, however, it is the public's desire to have cars produced that makes these activities possible.

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produce material means. Economic activity may be carried on to a greater extent than the interest in material means would justify, merely because it is regarded as pleasing to god (or because lack of such activity is regarded as displeasing to god) or as satisfying a moral requirement of being industrious; these motives have to be explained by sociology and philosophy. More frequently, a particular form of organizing economic activity may be chosen because it is considered morally superior or politically useful or necessary, although it is not the most effective in producing material means of want-satisfaction; the reluctance of Communist states to introduce market elements into their economic system is an example. Here, obviously, the political scientist should join hands with the economist to show why events go the way they do go.

There is no yardstick of judgment which would enable us to say that the production of goods and services is more important than the political, moral, or religious purposes which may require a particular extent, mode, or limitation of economic activity. Even more arbitrary is the attribution of goal-setting and pattern-maintenance functions to separate subsystems. Goals are set in the economy and in the family as well as in the polity, and here it seems impossible even to attribute primacy in any sense to one function of a particular subsystem. One of the complications of reality which stands in the way of the functional division attempted by Parsons and Smelser is the fact that nearly all means can also be ends in some context, and almost all ends can be means.

It would be unfair to accuse Parsons and Smelser of ignoring this and other complications. They are aware of them; they are not fully aware, however, of the extent to which these complications limit and partially destroy the usefulness of their approach. One may remember that when the successors of Ptolemy became aware that the apparent movements of the stars did not exactly conform to the cycles which the master had calculated, they assumed that every star did not move in a simple cycle only, but that its movement performed a cycle placed on the original cycle, an epicycle. In this way any complexity can be taken care of in a logically unimpeachable fashion, but the conceptual apparatus gets more and more complicated, and in spite of the complexity of the thought model, the latter cannot catch up with the complexity of

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institutions and processes. As a means to show some of the relationships between different areas of social reality, the Parsons-Smelser form of presentation can be very useful; as a means to describe the totality of these relationships, it is inadequate, and even more so as a means to merge the approaches of the various social sciences in the daily work of instruction and research.

The weaknesses of the Parsons-Smelser scheme are inherent in any attempt to impose an elaborate superstructure of concepts on those developed by the different disciplines. This is clear for the disadvantage of complexity, but it is also true of the incompleteness and arbitrariness of the scheme. Any such superstructure must have an ordering principle; in the case of Parsons-Smelser it is the functional imperatives of the subsystems and the boundary exchange between them, a principle that—as a total explanation—founders on the multiplicity of functional imperatives and on the multitude of interrelations.

Not only is the Parsons-Smelser scheme, like all the integration attempts of the same type, still incomplete despite its attempt at comprehensiveness; because of its inherent formalism it does not catch the most important substantive relationships among the areas of reality which are dealt with by economics, political science, and sociology. What can we learn from this scheme about the truth or untruth of the beliefs expressed in the slogan of the military-industrial complex, about the influence of economic policy on family life, about the effects of the economic system on foreign policy? The social scientist who has spent time and energy on learning the new concepts finds that these tools are of little use in understanding the most interesting connections between economic, political, and other aspects of social reality and that he has gained no more than the ability to make some generalizations of limited substantive significance, plus some occasional and not very distinct glimpses at features which might prove worthy objects of a unitary social science if they were taken out of the shadow of the formal apparatus.

Another book of the category which tries to integrate social science by building up superconcepts is Robert Dahl and Charles E. Lindblom's, *Politics, Economics and Welfare*.[11] This book is

[11] Robert Dahl and Charles E. Lindblom, *Politics, Economics and Welfare* (New York, 1953).

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primarily concerned with the integration of economics and political science, although incidentally the authors take in a good deal of sociology. The book says a good many things—a good many good things—about rationality, liberty, and economizing, but the central concept of the book is control: the material is ordered and analyzed according to the types of social control that are possible or that originate from various social processes. The resulting apparatus of concepts is formidable. Terms such as "manipulated field control," "spontaneous field control," and "reciprocity," the latter being used in a specific sense, are tools the application of which can be learned only with considerable difficulty; and when they have been learned, few if any social scientists will have enough time and energy left to employ them to elucidate the basic actual relationships between economic, political, and other societal phenomena. Moreover, the suitability of methods of control as the main ordering principle of societal phenomena is questionable. Of course, the means by which some individuals control others is a very important topic in all the social sciences, because this is a question of who exercises power over whom and by what methods. But there are relevant phenomena which can be fitted into that scheme only with difficulty, if at all. Not all that is important in society is domination. Equally important are cooperation and the construction of social mechanisms, such as the price system and the rule of law, which satisfy some societal needs by taking away or severely limiting any individual's power or discretion. Dahl and Lindblom, it is true, try to fit these phenomena into their scheme. Such concepts as "reciprocity," as a form of (mutual) control, and "polyarchy," as control of the leaders by the led, serve this purpose. There is nothing illogical about this terminology, but does it not obscure rather than reveal the most important aspects of the problem of power and its taming? The essence of the power problem is the domination of man by man; any classification scheme which emphasizes that control of the led by the leaders and control of the leaders by the led are both systems of control does not put the emphasis where it ought to be. If the Dahl-Lindblom book were merely an essay on the various methods of control, the scheme would be quite acceptable. But the book purports to create a general taxonomy of the most important societal phenomena, and for this purpose its classification criteria

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do not seem to be the most useful (even if we put aside the question of whether it is really taxonomy that we need most in unifying social science).

Just the same, if it is ever possible to develop a body of workable concepts covering the different aspects of societal life, as a tool to understand those aspects, such an endeavor will probably have to follow the steps taken by Dahl and Lindblom more than those of any other authors, however much simplification, shifting of emphases, and filling-in of loopholes may be required. The book brings out two basic facts with admirable clarity: first, that economic phenomena have the dual aspect of interpersonal relations and of the struggle against the resistance which the physical world offers to human want-satisfaction (sociologists often tend to neglect the latter and economists the former); secondly, that economic activity is a special effort to secure rationality in action and that therefore economics offers a prime illustration of the conflict between rationality and irrationality—a conflict which is an important part of human existence.

These examples may suffice to illustrate the nature and the weaknesses of attempts to build up a superstructure of concepts into which all societal phenomena can be fitted. Another book which undertakes the same attempt, also with a great deal of acumen and sophistication, and which is open to essentially the same kind of criticism as are Parsons-Smelser and Dahl-Lindblom, is *The Study of Society* by Alfred Kuhn.[12] It was more definitely intended as a textbook, and the cumbersome character of the terminology is therefore an even more important objection in this case than in the others; at the same time, like the other works in this category, Kuhn's book offers quite a few valuable individual insights.

II

Since the efforts to integrate the social sciences through a comprehensive common body of concepts have failed, a more modest approach seems in order: to discuss relationships between phenomena from different areas of societal life without—at least for the

[12] Alfred Kuhn, *The Study of Society: A Unified Approach* (Homewood, Ill., 1963).

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time being—aiming at comprehensiveness, and to form common concepts only to the extent that they are necessary to elucidate these specific relationships. This kind of endeavor is, of course, not new, but it seems that previous efforts have missed some central phenomena which ought to have been among the first to be analyzed from the point of view of several disciplines—or if not entirely missed them at least not carried the analysis far enough. Perhaps the most important of these phenomena is the economic market.

What is a market? The answer of earlier economists would have been: a place where suppliers and purchasers meet and bargain, setting prices through the bargaining process. Most present-day economists who have reflected on the nature of a market would give a somewhat different answer: a market is any arrangement for the meeting of supply and demand. Such an arrangement may be a physical market place where living persons meet as buyers and sellers or a system of communication facilities through which buyers and sellers, though physically distant, can let each other know of the quantities they would be willing and able to supply or purchase and of the prices which they would be able and willing to pay or expect to get; or the market may consist of the office of a planner who calculates from various statistics the supply that will be forthcoming and the demand that will exist for various commodities at various prices and who thereby arrives at a system of "shadow prices."[13]

In the present context the most important is the third possibility because, by making price formation independent of the system of ownership, it shows the general function of prices most

[13] For the gradual evolution of the idea of such a paper market with shadow prices, see Eduard Heimann, "Literature on the Theory of a Socialist Economy," *Social Research*, VI (1939), 88-113, and Carl Landauer *et al.*, *European Socialism* (Berkeley, 1959), chap. 46. But the broader concept of the market is still not universally accepted. Karl Polanyi and his associates, for instance, in their book *Trade and Market in the Early Empires* (Glencoe, Ill., 1957), denied the existence of price-forming markets in ancient Egypt and Mesopotamia mainly because archeological evidence does not seem to reveal the existence of a marketplace in the physical sense and because there are indications that the prices of merchandise were fixed by the government. But by what yardstick did the governments decide what the prices should be? Only if it could be shown that they were not influenced by the intention to equilibrate supply and demand could the Polanyi thesis be considered valid.

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clearly: to express the importance of goods and services and thus make possible, for each economic activity, a comparison of input and output. It then becomes clear that the formation of prices equilibrating supply and demand is an indispensable means, in all but the most primitive societies, to achieving a *rational*[14] use of resources, and thereby the task is set for various social science disciplines to describe what it means to live in a society with such an apparatus to assure rationality.

The phrase "prices equilibrating supply and demand" is not as unambiguous as it may look at first blush. There is, for example, the time dimension. Many resources used by man are exhaustible; depending on how seriously (expressed inversely, with what discount) we take the future as compared with the present—the evil that our progeny may be left without some types of resources—we shall attach to the using up of such resources in the course of present production a greater or a smaller significance as a cost factor. The time dimension is equally significant for the demand side: how much do we discount the value of some return that may come to us after a substantial lapse of time? This bundle of questions has particular significance because, generally speaking, we view the future with less discount in relation to the present when we judge as members of the community than when we judge as private individuals,[15] so the phenomenon of time preference is connected with the problem of the relationship of demand for the satisfaction of collective needs with the demand for the satisfaction of private wants and, indirectly, with the question of who determines the collective needs. (The time preference of the rulers may be different from that of the governed.) On the side of private wants, there is the problem of the extent to which they are spontaneous or have been manipulated, for example, by sales propa-

[14] This term is used here in the sense of rationality of means. In so doing we do not, of course, renounce the right to judge ends. Whether it is a commendable usage to speak of rationality of ends in regard to such judgments is a question of definition without relevance in this context.

[15] For an analysis of this problem, see William Baumol, "On the Social Rate of Discount," *American Economic Review*, LVIII (1968), 788-802; the comments on the Baumol article in the *American Economic Review*, LVIX (1969), 909-29, and LX (1970), 195-98; and the literature cited there. See also Harold M. Somers, "On the Demise of the Social Discount Rate," Paper presented at the annual meeting of the American Finance Association, Dec. 1970, with an extensive bibliography.

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ganda. The analysis of the determinants of supply and demand obviously requires the joint efforts of economists, individual and social psychologists, sociologists, and political scientists; it is here that simplified assumptions about human motives can be most misleading.

But whatever the factors determining the importance of cost goods and yield goods, the rule of rationality requires that no commodity unit be produced when the importance of the cost goods would exceed that of the unit itself. Whether prices are formed by bargaining buyers and sellers or by calculations of government planners, the price-forming apparatus will appear to the people whose lives it affects as a social mechanism which operates independently of any individual's will. In this respect it is similar to formal law without judicial discretion. Extreme formality of law (which has never existed in pure form) would make the judge an automaton into which facts would be fed and from which decisions would emerge. Similarly, where the price mechanism reigns supreme, such decisions as to continue operation of some plants or of whole industries would be found by a comparison of cost data and return data and not by the conscious will of any authority.

Before the objections to the rule of social mechanism are discussed, it is proper to consider its great value as a liberating factor. Formal law, in the modern sense, means equality before the law. It means that it is the human person himself, his actions and relevant qualifications, and not his status, race, creed, or wealth, which determines what the law does to him or for him; formal law also means that the particular moral or other preferences of the judge are superceded by general rules and that therefore the dependence of a contending or defending party on the man in the judge's chair is eliminated (or, in practice, minimized).[16] Likewise, under the rule of the price mechanism, decisions are objectivized: a worker will be hired if his employment will contribute more to the product, as measured in price terms, than the cost of his employment, regardless of whether his nose, his skin color,

[16] The freedom-protecting effect of formal law (with implications for the price mechanism) has been greatly emphasized and convincingly explained by Friedrich A. Hayek, *The Road to Serfdom* (Chicago, 1944), 79 ff., and *The Constitution of Liberty* (Chicago, 1960), 148 ff. Hayek, however, entirely overlooks the other side—the injustices and the alienating effect which formal law and an unrestricted rule of the price mechanism may cause.

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his political opinions, or his religion pleases the employer. A plant or an industry will be put or kept in operation if it shows an excess of product value over cost value and not otherwise, regardless of whether, for example, the government looks with favor or disfavor on the operation or on the people who undertake the operation.

Earlier in the modern age, when the arbitrariness of rule under feudalism and under absolute monarchy were vividly remembered,[17] the blessings of objectivity which the social mechanism tended to assure were considered a great achievement and met with relatively little criticism. It was even overlooked—at least most of the time—that prejudice and other factors always limited the operation of the price mechanism—that, for example, a tacit and sometimes even explicit agreement among employers might exclude Negroes, Jews, Irish, revolutionaries, or Catholics from jobs regardless of their qualifications and of the profit which might result from their employment.

The criticisms which the social mechanism has encountered since the latter part of the nineteenth century have to a considerable extent been directed against the results of the mechanism's operation, which are sometimes, but not always, the inevitable consequence of the nature of the mechanism. Take for instance the famous argument phrased by Anatole France: "The law in its majestic equality forbids the poor and the rich alike to sleep under bridges, to beg in the streets and to steal bread."[18] This injustice could be prevented if the law allowed the judge to take the economic position of the accused into account; but it could also be prevented by a distribution of income giving every person enough money to have decent shelter and adequate food.

The inequality of conditions which underlie the operation of the price mechanism have been extensively discussed by the institutionalist and historical schools of economics. Since these schools drew largely upon the results of research in psychology, sociology, political science, and history, their work has led to frequent, if rather unsystematic, cooperation among social sciences. To ad-

[17] It would be a great mistake, of course, to believe that formality of law or the price mechanism were absent under feudalism. But feudal law, although very formal in some respects, left more room for arbitrariness than nineteenth-century law, and the role of the price mechanism was smaller.

[18] Anatole France, *Le Lys rouge* (Paris, 1969), 75.

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vance, or revive, this kind of cooperation in elucidating the factors which ultimately determine supply and demand will remain an important task of social scientists, but it is not the task on which the remainder of this essay will concentrate. What will be discussed here is not primarily the fact that the same kind of social mechanism when applied to people of different backgrounds, traditions, psychic needs, and so on may lead to very different results and frequently to injustices and other inadequacies; that, for instance, the rules providing for equal right to be admitted to higher education may *de facto* discriminate against ethnic or other groups whose opportunity to acquire sufficient preparatory education has been limited; or that a penal code equally applicable to all citizens may fail to realize equality before the law if, for some groups of citizens because, for example, of their economic position,[19] the temptation to break the law is overwhelmingly greater than for others. Also, only in passing shall it be noted that an absolute rule of the price mechanism tends to lead to an inadequate satisfaction of collective needs, because their importance is more difficult to quantify and thereby to fit into a calculation with value magnitudes. All these limits of the usefulness of the price mechanism and of formal law have implications which can be analyzed only through the cooperation of several social sciences. But they spring from the environment in which the mechanism operates; what is to be discussed here are the positive and negative effects of the social mechanism itself, the pros and cons of the rule of law in the broadest sense, not only in jurisprudence but also in economic life, in order that the need for a joint effort of the social sciences in presenting the positive and the negative side may become clear.

[19] Differences in the economic position which lead to actual inequalities in the application of formally equal law offer only one example among many. A law which imposes military service equally on all able-bodied citizens of defined age-brackets causes agonizing dilemmas for the pacifist, but only physical hardships for the man whose opinions agree with the philosophy behind the law. Formal requirements for divorce may heavily discriminate against an especially sensitive person who finds it unbearable to remain married to a spouse with whom he or she has no longer complete community of views and feelings—a degree of harmony which may not seem so essential to the majority of citizens. Obviously, if the liberty-protecting effect of formal rules is not to be sacrificed entirely, it is impossible to relax formality to the extent that all such *de facto* inequalities are eliminated.

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III

There is one basic fact that provides an appropriate starting point for all such considerations: men do not like to be subject to arbitrary personal rule, but neither do they always like to be governed by anonymous social machinery. The impossibility of arguing or pleading with an individual who has the power of setting things right—as he has the power of doing wrong—can be a severe frustration. Anonymity of regulation, as well as arbitrary rule, can cause a feeling of powerlessness leading to the kind of unhappiness which is often called alienation. On the other hand, the amount of social discipline necessary for life in society can only be assured either through the power of a ruler (or ruling group) or through a social mechanism or through a combination of the two. Since personal rule implies arbitrariness, the only way to reduce arbitrariness is a total or partial substitution of social mechanism for personal authority. Practical wisdom has led men to the avoidance of extremes: we have social mechanisms which are sufficiently tight to prevent an excess of arbitrariness but not so tight as to exclude all discretion by those who can lay their hands on the levers of the mechanism—discretion, which can be used to humanize the apparatus, and simultaneously creates the danger that some arbitrariness will creep in. Yet the cooperation of the social sciences which would be necessary to elucidate the roots of the dilemma and the usefulness and the limitations of the compromises has not been forthcoming to a sufficient extent. One of the reasons for this deficiency in cooperation is the assumption implied by many social scientists that man, though perhaps not quite omnipotent in choosing ends and means for social transformation, does at least not meet with severe limitations in shaping the society which he has created. From this attitude results a hesitancy to take dilemmas seriously and to cooperate in finding the least undesirable solution.

This overestimation of the pliability of social relations and institutions is illustrated, for instance, by the tendency of many psychologists, and following them of some sociologists, to speak as if social institutions and their mode of operations could and must be adapted, almost without qualification, to the psychic needs of the individual; economists and political scientists, on the other

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hand—except those who have been swept off their feet by the pseudo-revolutionary, antiauthoritarian trend—know that institutions are not infinitely pliable and that an attempt to give the individual unlimited leeway—even if this were optimal from the viewpoint of individual psychology—would ruin the institutions which are necessary to protect the material foundations of human life.[20] In this way a sort of ideological conflict has arisen between the approaches of many sociologists and the approaches imposed on economists and political scientists by the nature of their disciplines. If the conflict were really fought out, it might in the end draw the disciplines together; but so far there is not much of a true debate, either side being essentially satisfied with cultivating its own point of view.

One form of the failure to recognize the constraints under which mankind labors in shaping its societal life, and the resulting dilemmas, is the gratuitous assumption of many noneconomists

[20] Erich Fromm's *The Sane Society* (New York, 1960) may serve as an example, just because he is not quite as extreme as some other scholars on the borderline of sociology and psychology. Unlike the spokesmen for the youth movement against the "consumer society," the "corporate state," and the like, Fromm recognizes that the requirements of societal life put some limits on the adjustment of institutions to the conditions of individual well-being, but he bridges the gap between the possible and the desirable by wishful thinking. In arguing for the feasibility of "communitarian socialism," for instance, he cites the example of a French watchcase factory organized as a sort of collective; he gratuitously assumes that the giant enterprises in which much of modern production must be carried on can be so decentralized as to permit the realization of the same principles. He wants to make economic activity "a subordinate part of social life" (p. 326) without giving thought to the tremendous economic tasks which his own postulates—social justice, assimilation of the living standards of nonindustrial with those of industrial countries, to which protection of the environment would presumably have to be added—will set for mankind. He recommends steps to curb undesirable features in the "movie industry, the comic-book industry and the crime pages of our newspapers" without even raising the question whether such censorship will not lead to an infringement of the freedom of expression. He wants production and living units small enough to apply the communitarian principles which he advocates, but he lays a ban on "local patriotism for the firm . . . an 'esprit de corps' similar to that of college and university students," because such feelings "would only reinforce the asocial and egotistical attitude which is the essence of alienation" (pp. 325 f.), although it is certain that small units need for their functioning the cohesion which only "local patriotism" can give. In other words, he very much underrates the importance of incompatibilities in social change and, therefore, the amount of things unwanted which we must accept in order to get what we want.

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that scarcity is a thing of the past, that we can now produce enough to satisfy all "genuine" wants even if we relax our efforts to secure rationality in production;[21] economists have not done nearly enough to convince their fellow social scientists that even if this were true of individual wants, it would not be true of collective needs.

If we were free from this and other constraints, the dilemma of personal rule versus rule by social mechanism would be less acute, if it existed at all. The constraints, those, for example, which result from the continuing need for rational production, require a high degree of social discipline. If this requirement did not exist, we could relax all forms of rule enough to make the burden of any form easily bearable. Because we need strong social discipline, we have to accept some undesirable effects either of personal rule or of rule by social mechanism or of a mixture of both. If we want collaboration among social scientists to find the mixture with the least undesirable effects, or other ways to make the dilemma bearable, the first prerequisite is to convince all concerned that we really have to choose between alternatives, each of which has a negative side.[22]

[21] See, for example, the defense of the hippies by Theodore Roszak, *The Making of a Counterculture* (Garden City, N. Y., 1969), 36, against Professor Hans Toch's criticism of them for being "consuming but noncontributing"; Toch, "Last Word on the Hippies," *The Nation*, Dec. 4, 1967, pp. 582-88. Roszak thinks the hippies are justified in living this way because "we have an economy of cybernated abundance that does not need their labor, that is rapidly severing the tie between work and wages, that suffers from hard-core poverty due to maldistribution, not scarcity." Aside from his error that present-day society has a permanent superfluity of labor—not only of unskilled labor and of some types of skilled labor in times of recession or in special cases of technological obsolescence of skills—he fails to consider that maldistribution of income is to a large extent a consequence of continuing scarcity. If the economy were so productive as to generate a greater amount of public revenue, it would be much easier to enhance the earning capacity of those who now lack the chance of earning decent wages because of poor education and other forms of cultural deprivation.

[22] In addition to their overestimation of present-day affluence, another misconception of some sociologists aggravates their opposition to the idea that more product and therefore more work is still needed, and thus increases the difficulty of their collaboration with economists. A strawman of "technocratic" or "middle-class" society is built, based on the assumption that joyfulness of life is essentially contrary to production by modern means. This is in large part a confusion between the dynamics of history and the mechanics that preserve an achieved level of performance, including its apparatus for further development. Puritanism may have played an indispensable role in overcoming

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The relationship between social mechanisms and personal power is complicated by the possibility, indeed the probability, that the power-taming effect of the social mechanism may leave loopholes. A power-greedy individual may then get hold of the mechanism and manipulate it in his own interest. Something like this has happened under fascism. It is also clear that in some situations, power can be exerted through the economic market. Sociologists have, on the whole, been inclined to magnify the role of power in a market economy; so have those economists who were under the influence of Marxism, whereas the rest of the economics profession has largely ignored the power problem. What most of the sociologists have failed to study is the diversity and frequent ambivalence of economic interests which make the exercise of power through the economic apparatus more difficult. C. Wright Mills, for instance, in his famous book *The Power Elite*, has described the interlocking leadership in economic life and government and concluded: "The top of American society is increasingly unified, and often seems willfully coordinated: At the top there has emerged an elite of power."[23] Although leading posts in government and business are indeed to a great extent filled by people of roughly the same background and an interchange of personnel does take place among the various fields of decision making, yet the elite, especially the business elite, is anything but a unified entity. It contains parts which are interested in armament expenditures and others which are interested in concentration of public funds on such purposes as housing, highway construction, education,

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the tremendous obstacles to the emergence of a business civilization; this does not prove that this civilization can only continue to exist on the basis of a puritan mentality. "To liberate sexuality would be to create a society in which technocratic discipline would be impossible," says Roszak, 14, paraphrasing Herbert Marcuse. Why? Any pleasures that do not destroy working ability, as do continuous drunkeness or drug addiction, are compatible with an advanced technological society. (So are communes with or without promiscuity, or niches for those who reduce equally their consumption and their work load.) In the language beloved by Roszak and other dissenters, there is no conflict between Dionysius and Hephaestus.

[23] C. Wright Mills, *The Power Elite* (New York, 1956), 324. Of course, not all sociologists have adopted the concept of a unified power elite. David Riesman's idea, expressed in *The Lonely Crowd* (New Haven, 1953), 244 and *passim.*, that there are many "veto groups" in American society whose political influence tend to prevent decisions which are too one-sided implies the diversity of economic interests which really exists.

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and others; sections which are interested in high tariffs and others interested in commercial agreements for the lowering of tariffs; some which are interested in price stabilization at the price of restricted growth and still others interested in the promotion of economic growth even if this means inflation. There is still something like a common front of business against high taxation and against legislation favorable to labor unions, but especially the latter interest, which in the past formed an important rallying point for business, has lost much of its strength, because unions are now taken for granted and the issues concern only marginal expansion or restriction of their power.

The conflicts of interest within the business world and their impact on politics and on the relations of business with other sectors of society would offer an important field to a unified social science, but the first condition of success in such an endeavor is the liberation of the investigators' minds from those preconceived ideas which have survived from a time when the lines of the class struggle were more clearly drawn. The same holds true of problems raised by the imperfectness of competition. Economic research of the last forty years or so has shown that this imperfectness is much more pervasive than most economists had previously assumed. It is not quite so long ago that some researchers found that the assumption that profit maximization is the exclusive goal of corporation management is not generally valid. Since managers, operating under imperfect competition, have a margin of profit which—within limits—permits the pursuit of other goals, such as empire building for its own sake, without serious danger of bankruptcy, the motivations of managers have become a worthy object of research, but sociologists have shown little interest in this problem. Here again, preconceived ideas have probably been an obstacle. The devil's legends about monopoly capitalism have not been conducive to an investigation with open minds, or even to the realization that the corporate structure of our economy should not be just a target for political attack but a source of questions for scholars to solve. Useful cooperation between economists and sociologists in the analysis of economic power presupposes the abandonment by the latter of some ideas about economic life which are as oversimplified—though of a political color on the opposite side of the spectrum—as those of the most naive laissez-faire economists.

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Since corporation management can pursue goals other than profit maximization, the question comes up whether it should not be committed to goals of welfare policy in a broad sense, from environmental protection to the promotion of racial equality. What is at stake here is goal setting by corporation managers, not imposing constraints on these managers by legislation or public administration. For a long time governments have assumed the right to forbid industrial practices harmful to health or public welfare, although the comprehensiveness of the regulations and the vigor of their enforcement have fallen far short of what is needed today. But recently the demand has been raised that business enterprises themselves, on their own volition and independent of government orders, assume the obligation to realize public welfare goals. This demand raises the question of the merits and demerits of functional division in society. Our society has so far been organized on the principle that individuals and institutions have specific functions and each should concentrate on their fulfillment, leaving the fulfillment of other functions to those within whose commitments these functions lie. So far, the sociologists do not seem to have realized that in the commitment of private enterprise to goals different from profit maximization the problem of functional division is involved, and that they should get together with the economists to work out a tenable opinion on whether or not the commitment to specific functions should be more qualified than it is now.[24]

IV

These points have been discussed here to show that a number of real and urgent problems require the collaboration of social scientists from different disciplines, and that for this purpose only a minimum of new superconcepts is required, whereas up to now the main effort in bringing the various disciplines together has been through the development of new taxonomies which would embody all the concepts of the individual social sciences and the underlying phenomena. The list of problems which require collaboration is, of course, in no way exhaustive; not even all aspects

[24] It is perhaps even stranger that the controversy over the right or duty of universities and professional organizations to take a stand on political issues has been conducted with very little reference to the general theory of functions, although the question of whether it is within the tasks of the university to

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of the relationships which have been mentioned have been fully worked out. For instance, the problem of social mechanisms versus personal rule has also great significance from a public administration point of view. Executive discretion is not always a boon but often a burden, and it is frequently useful and even necessary to relieve administrative agencies of much of that burden; the advocates of (absolute or qualified) laissez faire have often argued that only by leaving most or all of the economic decision-making to the market can the machinery of government reserve its energies for the proper tasks of state policy. Although this argument has largely camouflaged other motives for opposing state intervention, the capacity of social mechanisms, like the market, to take over tasks which otherwise would have to be solved by government decision deserves to be thoroughly examined by those interested in the efficiency of the government apparatus. The recent experience of the Soviet-bloc nations, that only by enlarging the role of the economic market in decision-making can an unworkable degree of administrative centralization be avoided, should not be a matter of interest merely to Sovietologists; it contains some general lessons.

There is a dialectic in the development of scholarly endeavor as there is in many other human activities: the more we increase specialization, the more we are in need of synthesizing efforts. Synthesis, as well as specialization, has its methodological problems. They have to be consciously faced: criteria can be developed by which the most useful approach is chosen, or at least by which the less useful approaches are eliminated. A mere drifting in different directions, determined by the individual writers' preferences untested by serious examination, is not likely to be fruitful enough to create a counterweight to the powerful forces which are driving us in the direction of ever-increasing specialization.

take such a stand has often enough been raised. To be sure, the customary statement that the function of the university is to expand and disseminate knowledge takes too little account of the fact that in many of its parts, especially in professional schools, practical skills are being taught which require a value orientation. But to recognize this fact does not necessarily imply that the university should not operate on the basis of a specificity of function which excludes political commitments. Probably no function of any class of individuals or any institution can be so defined as not to appear, so to speak, fuzzy at the edges; but this is no good reason to reject functional division as such.

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Local Leadership and the Typology of New England Towns, 1700-1785

EDWARD M. COOK JR.
University of Chicago

In recent years, students of colonial New England society have produced a large number of studies of local communities which seek to describe the social structure, community ethos, and political structure of the area by presenting one community as a microcosm, in one sense or another, of the general society. As most of these scholars admit, however, their ability to generalize is severely hampered by the absence of studies providing comparative data for a number of communities and establishing any kind of conceptual framework for explaining how the experience of one town might relate to that of another. At the same time, a substantial debate has been raging over what the nature of the political system was, specifically whether the system was "democratic" or "consensual," both of which terms refer to an egalitarian political order, or "oligarchic" or "deferential," terms which refer to more stratified orders.[1] All of these contentions are supported to

[1] For example see Robert E. Brown, *Middle Class Democracy and the Revolution in Massachusetts, 1691-1780* (Ithaca, 1955); Michael Zuckerman, *Peaceable Kingdoms: New England Towns in the Eighteenth Century* (New York, 1970); J. R. Pole, "Historians and the Problem of Early American Democracy," *American Historical Review*, LXVII (1962), 626-45; Joel A. Cohen, "Democracy in Revolutionary Rhode Island: A Statistical Analysis," *Rhode Island History*, XXIX (1970), 3-16; Carl Bridenbaugh, "The New England Town: A Way of Life," *Proceedings of the American Antiquarian Society*, LVI (1946), 19-48.

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a considerable degree by statistical data which are hard to evaluate because of their fragmentary nature.

Using partial data and preliminary findings from a broader study of local leadership in seventy or more eighteenth-century towns drawn from all four New England colonies, I would like to suggest a frame of reference for the interpretation of statistical data on political behavior in the towns and, insofar as a typology based primarily on political behavior can suffice, for the overall interpretation of community studies. To do so, it will be necessary not only to develop a series of indices of political behavior in the towns and to describe in detail the attributes of towns of various types, but also to support these efforts with an analysis of the relationship between the town political structures and the political power of the provincial elites.[2]

I

At the outset, a study of local leadership faces a problem of definition. No town designated "leaders" as such, of course; leadership is a relationship among people, not a formal position. In practice, however, town leaders were the holders of the chief local offices. The selectmen, or as Rhode Islanders called them, town councilmen, were by definition the most trusted and influential townsmen, and the few prominent individuals who eluded that office invariably served in the equally important offices of town clerk, town treasurer, representative, and moderator. Taken together, the holders of these five offices constituted a clear group. In the seventy-odd towns studied, about 85 per cent of the whole group served as selectmen, including fully three-quarters of those who held the other four offices. These leaders served in other offices as well; they were most frequently assessors, highway surveyors, and members of special committees, but for no other widely established set

[2] The data on the 39 towns presented in the table were selected for illustrative purposes from a larger study and are not intended as a statistical "proof" of the hypotheses about political structure and practice offered here. The argument sketched here is more fully presented in Edward M. Cook Jr., "Local Leadership Patterns in Eighteenth Century New England" (Ph.D. diss., Johns Hopkins University, forthcoming), where space allows not only appropriate statistical verification of the quantitative data, but also a thorough development of the spatial and temporal variations in the hypothesis as well as of its relation to broader aspects of social and political behavior.

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of offices was the degree of overlap as consistent or as large.[3]

In several respects, towns throughout New England chose similar kinds of men as leaders. Typically, they were in their 40s and 50s, having first emerged as leaders shortly after 40—age at first service averaged 42.6 years—and they finished their careers by the age of 60. They had prepared for major office by serving several years in lesser offices, such as fence viewer, constable, or highway surveyor. Usually, too, they were among the wealthy inhabitants of their towns, because extensive service in unpaid public offices required at least a moderate amount of freedom from daily labor.

In other ways, town electoral practices varied markedly. One significant area of difference was in the number of terms a single leader served during his lifetime. In some towns, leaders served only three or four terms apiece and a large percentage of the male inhabitants served as leaders. Historians frequently cite behavior of this sort as a sign of widespread participation in an egalitarian (or as some would extend it, democratic) political order. But in other towns, leaders served as many as seven or eight terms on the average and relatively few inhabitants ever became leaders. Towns with such histories have provided evidence for historians who argue that political participation was effectively limited there to a class of gentry who ruled as part of an aristocratic order.

If length of service and breadth of participation in major offices are used to test political structure, there is evidence of both egalitarian and restricted orders, as the data in columns 3 and 4 of the table illustrate.[4] Interpretation of town politics is not a question

[3] The group definition of the pool of town leaders is more fully explained in "Local Leadership Patterns." The towns surveyed in the larger study are—MASSACHUSETTS: Amherst, Andover, Barnstable, Barrington, Billerica, Boston, Boxford, Braintree, Brookline, Cambridge, Dedham, Dorchester, Duxbury, Fitchburg, Grafton, Hadley, Hanover, Hardwick, Hatfield, Leicester, Lunenberg, Manchester, Medfield, Middleboro, Norton, Oakham, Pelham, Shirley, Springfield, Sudbury, Swansea, Topsfield, Waltham, Watertown, Westminster, and Worcester. MAINE: Biddeford, Brunswick, Gorham, Newcastle, and Scarborough. NEW HAMPSHIRE: Bedford, Canterbury, Chester, Concord, Cornish, Derryfield, Dover, Dublin, Hampstead, Hampton, Hanover, Londonderry, New Ipswich, Portsmouth, Weare. RHODE ISLAND: Exeter, Middletown, Providence, Smithfield, South Kingstown. CONNECTICUT: Ashford, Bolton, Derby, Enfield, Hartford, New Haven, Newtown, and Stonington.

[4] The percentages in column 4 of the table, of course, approximate their literal meaning of a comparison between the leader pool and the population

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of validating one hypothesis and rejecting the other, but rather of deciding which hypothesis, with suitable refinements, best fits the town or group of towns under discussion. For one group of large towns—Boston, Braintree, Norton, and Springfield, for example—participation was narrow and individual service was long. Many of these towns are on or near the coast, but others are scattered throughout the interior, so that their similar behavior cannot be explained by regional peculiarities. For another group of towns, most of which were small, participation was broad and individual service short. These towns—which include Boxford, Grafton, Medfield, and Pelham—are equally scattered. A few towns deviated from the overall pattern, but most of these were newly established frontier towns like Fitchburg and New Ipswich or towns in peculiar circumstances like Brookline, a residence for wealthy Bostonians, or Amherst, newly separated from the important Connecticut valley town of Hadley.

Neat as these results may appear, however, they do not constitute a conclusive measure of the political order in the towns. Averaged figures, such as those used so far, tend to minimize differences: although the average length of service may be similar for a group of towns, it is possible that real power relationships, such as the disproportionate influence a man who stayed in office for ten or twenty years could acquire, would vary widely. The figures in column 6 of the table, which are a calculation of the percentage of all terms as selectmen served by men who served five or more years, were gathered in an effort to measure this kind of phenomenon. Clearly, the results complement the earlier findings. In the smaller, more egalitarian towns, like Boxford or Grafton, less than half of all terms were served by the men who served five or more years; in the larger, more restrictive towns, like Boston, Andover, or South Kingstown, between two-thirds and three-quarters of all terms were served by such leading officeholders. Once again, newly incorporated towns, like Fitchburg, Newcastle, or New Ipswich, failed to show any consistent pattern.

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from which it is drawn only for towns which were founded in the last generation of the colonial period. The intent here is to establish a rough guide to the breadth of the leader pool in general relation to population size rather than to create a precise index.

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Still another index to political behavior in the towns can be obtained from an examination of the office-holding patterns of sons and other near relatives of prominent officeholders. If, regardless of the normal distribution of offices, the sons of officeholders were especially powerful, it would seem safe to conclude that family connections gave real advantages and that access to office was stratified. If, on the other hand, powerful men in one generation had no marked connection with powerful men in the previous generation, access would seem to have been relatively open.

The question raised here has very complex ramifications, and, unfortunately, the answer cannot be conveniently set down in a brief table. As a general rule, the major offices in the New England towns were not the exclusive preserve of any small group of families. In virtually every town new immigrants and men of obscure origins found their way into important offices. Variations did exist, however, in the freedom of access to influential positions. In the largest towns, like Boston, New Haven, and Portsmouth, New Hampshire, recruitment of leaders was generally open, although the overall office-holding pattern was one of long terms and restricted participation. Family dynasties did exist, but for most officeholders status in the mercantile community or alliances in factional politics, both of which were notoriously transitory, were more important avenues to power.

In most of the other large towns, however, family connections virtually guaranteed some men entry into major office. In Cambridge, for example, three successive Andrew Boardmans served as town clerk and town treasurer almost continuously from 1700 to 1779 and served as well a total of 72 years as selectman or representative. In Dedham, three successive William Averys were leading selectmen, as were the three successive eldest sons of the Daniel Fisher and Joseph Ellis families. These were eastern towns, but the pattern was equally strong in inland towns. In Worcester, for example, John Chandler and his two sons served 149 terms in major offices between 1733 and 1775; in Norton, members of the Leonard family served one-quarter of all terms in the major offices; and in the Connecticut river towns, the Pynchon, Williams, Partridge, and Porter families were equally powerful.[5]

[5] Lucius R. Paige, *History of Cambridge, Massachusetts, 1630-1877* (Boston, 1877), 459-69; Dedham Town Records, V-VII; Worcester Town Records, *Col-*

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By contrast, in towns where offices were widely distributed, access to office was essentially unstructured. Sons of leaders often became leaders, but so did virtually every other reasonably capable individual. Moreover, sons of leaders showed no special tendency toward long service. In little Medfield, for example, 50 of 142 selectmen were sons of leaders, but no son served more than 9 years as selectman and only 4 served more than 10 years in all major offices combined. As a group they served an average of 4.9 years, somewhat less than the average of 5.6 for all of the leaders. A similar situation existed in Grafton. There 29 of 93 major officeholders were sons of leaders. One of these sons served 20 terms, making him a prominent leader, but, as a whole, the 29 averaged 4.5 years of service, a figure virtually indistinguishable from the overall average of 4.46.[6] This pattern of wide access to office existed in frontier towns as well as in the established towns discussed here, although in the frontier town this openness was an aspect of the town's recent settlement, not a significant feature of a developed political structure.

Evidence of differentiation in length of service, breadth of service, concentration of power, and access to office suggests three main political patterns in the towns: an erratic frontier pattern, an egalitarian small town pattern, and a stratified large town pattern within which there is a hint of a distinction in access to office between the largest cities and the rest. If this is true, we might reasonably expect that the variation in political structure would find some reflection in the social structures of the towns. The analysis of social structure is, of course, a complex problem; but a beginning can be made with the convenient, if unsophisticated, technique of using the distribution of property as an approximate index of the stratification of society. Such an index appears in column 5 of the table.[7] The figures there represent the percentage

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lections of the Worcester Society of Antiquity (Worcester, 1881-82), II-IV; George F. Clarke, *History of the Town of Norton* (Boston, 1859), 280-300.

[6] William S. Tilden, *History of the Town of Medfield, Massachusetts, 1650-1886* (Boston, 1887), *passim.*; Frederick Clifton Pierce, *History of Grafton, Massachusetts* (Worcester, 1879), 409-32.

[7] Only data from the period 1750-75 appear in the index of stratification because the concentration of property increased over the course of the century for towns of similar size, age, and state of development. Data from earlier periods, therefore, must be compared with contemporary figures to yield meaningful results. For similar measures of social stratification, see Jackson Turner

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of the town's taxable property owned by the wealthiest 10 per cent of its taxpayers for towns which have left tax lists from the third quarter of the eighteenth century. These figures must be viewed with caution—assessment procedures varied from town to town, not to mention from colony to colony, and a suspicion lurks that some towns may have seriously underassessed their wealthier inhabitants—but the tax lists can be used at least as a guide. In general, the social structures suggested by this index agree with the patterns of political structure discussed in the preceding paragraphs. For the larger towns, where restricted participation and concentrations of power suggest a stratified political structure, the concentration of property is significant, suggesting a stratified social structure as well. In Boston, the wealthiest 10 per cent owned 55 per cent of the taxable property, and in most of the other towns in this group they owned 35 per cent or more of the property.

For the towns where participation was broader, the evidence suggests that the social structure, too, was less stratified. In none of these towns did the richest 10 per cent own more than 35 per cent of the taxable property, except possibly in Bedford, New Hampshire, where the concentration of property took an inexplicable jump from 25 per cent on a list of 1750 to 42 per cent on a list of 1770. More typical, however, were towns where the richest 10 per cent held slightly more than one-quarter of the property—such as Medfield, where they had 28.5 per cent; Pelham, where they had 28.5 per cent; and Bolton, where they had 30.8 per cent. According to this measure of social structure, the frontier towns also behaved predictably: virtually all of them had less than 35 per cent of their wealth in the hands of the richest 10 per cent.

II

So far, this discussion has dealt exclusively with the internal structures, political and social, of towns. But the towns were parts of larger entities, the colonies, and the political order of the larger

Main, *The Social Structure of Revolutionary America* (Princeton, 1965); and James T. Lemon and Gary B. Nash, "The Distribution of Wealth in Eighteenth Century America: A Century of Change in Chester County, Pennsylvania, 1693-1802," *Journal of Social History*, II (1968-69), 1-24.

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society had a direct relation to the order in a town. One way of studying this relationship is to look at the local origins of colonial officials and to try to assess the relative importance of various towns in the overall political structure. Whether or not a town produced leaders of sufficient consequence to hold provincial office and how high those leaders progressed are, like the internal structures of the towns, important indicators of local patterns in the political order.

In an attempt to discover these patterns, I have identified the holders of several levels of provincial offices by place of residence and, to a lesser degree, by family ties and social and political connections. The first kind of official examined was the justice of the peace, or in the case of Rhode Island and Connecticut where justices were appointed for every town regardless of size and importance, the justices of the county court. These officials were the most numerous and widespread class of provincial official, and on the lowest level. A second level of office was the higher judiciary, represented in this study by the justices of the Superior Courts in New Hampshire, Rhode Island, and Connecticut and by the justices of the Inferior Court of Pleas in Massachusetts. Selection to these offices, which was not made on the basis of legal expertise, depended upon considerable influence within the provincial elite, and appointment indicated that a man had achieved considerable prominence. The third, and highest, group of officials studied are the councilors or assistants in the four colonies. Membership in the council was a high honor; and since all of the colonies, except New Hampshire in the second half of the period, sought to achieve geographic balance in selections, the composition of the councils is a good measure of the distribution of power at the top of the political structure.

For the moment, the issue of whether provincial officeholders achieved their position through influence acquired as town leaders or through connections of another sort must be postponed. It will be enough to rank towns by the number of prominent individuals living in them, because it seems undeniable that the relative prominence of the town's political leaders would have a bearing on variations in political structure. Column 7 of the table presents an index of the prominence of a town's leaders. To compute this index, the number of justices of the peace, judges, and councilors

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resident in a town were summed and the number of college graduates among the leading town officers added, because a college education provided a man with powerful contacts in elite circles. The total was then divided by the population of the town in 1765, so that the figures in the table indicate the number of prominent men over the period down to 1775 in relation to one thousand inhabitants in the census.

Overall, the towns fall into three categories. First is a class of small or moderate-size towns with an index of five or less. Some of these towns, like Pelham or Bolton, contained no prominent men at all, although most had a handful of justices of the peace and college graduates. Judges and councilors rarely came from towns of this type. Next is a class of towns, usually of large or moderate size, in which the index ranges between five and ten. These towns had a substantial number of justices and college graduates and usually one or two men who achieved the rank of judge or councilor. Towns of the third class, with an index of ten or more, were the leading towns in the colony and contained substantial numbers of judges and councilors as well as numerous justices and college graduates. Boston and Portsmouth, both provincial capitals, and New Haven, a half-capital, belonged to this class, as did county seats like Cambridge, Springfield, and South Kingstown. But not all such major towns were government centers, as the example of towns like Braintree, Hadley, and Hatfield reveals. These major towns, like the towns in the other two categories, were distributed widely throughout the New England countryside.

III

Adding the classification of towns according to the prominence of their inhabitants to the earlier results of the effort to rank towns according to breadth of participation in their leadership, concentration of power in the hands of the most active leaders, freedom of access to office, and stratification of the socio-economic structure, provides five different sets of categories for the social and political behavior of the New England towns. Under close examination, however, this kaleidoscopic pattern of small towns, open towns, prominent towns, stratified towns, and frontier towns

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resolves itself into a remarkably simple typology. Since the recitation of rankings and groupings thus far has been complicated, I would like to describe in some detail the five major types of towns that emerge, even though to do so will necessitate a certain amount of repetition.

Cities. The first major type of town, represented in the table by Boston, Portsmouth, and New Haven, was the city or urban center. In socio-economic terms, these towns were highly developed, with a substantial concentration of wealth in the hands of the wealthiest inhabitants and a complex economy based on commerce and, to a lesser extent, on the presence of governmental agencies. The local political structure was also fairly elaborate. The group of town leaders was numerically large, although restricted, and, in contrast to leaders in the other types of towns, tended to specialize in offices. In Boston, for example, 40 out of 86 representatives never served as selectmen.[8] Power was concentrated, at least in the sense that leaders with five or more terms of service served about two-thirds of all the terms; but entry into office was open to virtually any man of wealth and ability willing to devote his time to the town's service. Town office was therefore a significant vehicle for mobility.

The cities provided a large percentage of the provincial officials, including many of those at the highest levels. Many provincial officials had held major town offices at some stage in their careers, but a major segment of the group had not. Some of these were undoubtedly excluded because of the opposition character of capital-city politics, which in Portsmouth and Boston, at least, was similar to eighteenth-century London politics; but other members of the provincial elite undoubtedly held themselves to be above town politics.[9]

County Towns. A second type, which might be called the major

[8] Robert Francis Seybolt, *The Town Officials of Colonial Boston, 1634-1775* (Cambridge, Mass., 1939).

[9] For Boston, see G. B. Warden, *Boston: 1689-1775* (Boston, 1970); for Portsmouth, compare the lists of representatives scattered throughout the *Provincial Papers of New Hampshire* (Manchester, Nashua, and Concord, 1869-1874), III-VIII with the account of politics in Jeremy Belknap, *History of New Hampshire* (Philadelphia, 1784-92), and with the biographical sketches in Clifford K. Shipton, *Sibley's Harvard Graduates* (Boston, 1933-69), for such men as Henry Sherburne (VIII, 490-98), Nathaniel Rogers (VI, 213-14), and Benning Wentworth (VI, 113-130).

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county town, consisted of towns which were local social and economic centers, and often county seats, but which were not large enough to have a definite urban character. These towns were scattered all over New England; some of them like Braintree and South Kingstown are near the coast, but others, such as Springfield and Worcester, are well inland. They had a distinctly stratified social structure, with significant concentrations of property in the hands of the wealthiest 10 per cent at one end of the spectrum and a visible class of landless poor at the other end. A relatively limited group of leaders occupied the chief offices and compiled a high record of average service. Like leaders in the cities, those who served five years or more served two-thirds or more of all the terms, but unlike the urban pattern, family continuity limited mobility in these towns. The local elite was often headed by one or two preeminent families, whose members served only in the highest local offices, some of which they treated as private family preserves. The Leonard family, which served one-quarter of all terms in major office in Norton, the Pynchon family, which served one-fifth of the terms in Springfield, and the Partridge-Williams family, which served more than one-third of the terms in Hatfield, are spectacular, but not unrepresentative examples.

Led by these prominent families, the local leaders in major county towns dominated the provincial offices in their counties. In Bristol County, Massachusetts, all the county judges and all the councilors after 1746 lived in the major towns of Norton, Taunton, and Dartmouth; in Middlesex County, three of the four county judgeships were reserved for inhabitants of Cambridge and Charlestown; and in Kings County, Rhode Island, over 80 per cent of the judges and assistants lived in North Kingston and South Kingstown. Almost without exception, the men who filled provincial offices were town officials as well.[10]

Suburban Towns. In a third, distinct class were towns, such as Dedham or Brookline, which functioned as suburbs to the cities or towns, such as Hardwick, Swansea, or Stonington, which functioned as secondary centers in the rural counties. These towns had

[10] William H. Whitmore, *Massachusetts Civil Lists for the Colonial and Provincial Periods* (Albany, 1870); Emory Washburn, *Sketches of the Judicial History of Massachusetts* (Boston, 1840); Joseph Jenckes Smith, *Civil and Military List of Rhode Island* (Providence, 1901), I; John Russell Bartlett, *Colonial Records of Rhode Island* (Providence, 1855-67), II-X.

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a fully developed social structure, with one-third to one-half of the property in the hands of the richest 10 per cent and a substantial number of landless poor. As in the major county towns, a well defined group of leaders occupied the principal offices, with significant family continuity, a concentration of power in the hands of those who served the longest, and a high rate of average service. In contrast to the pattern in the major county towns, however, there were usually at the top of local society a half-dozen families roughly equal in wealth and prestige, no one of which dominated local politics. The members of these families were prosperous yeoman farmers, local tradesmen, or, at most, very minor gentry. For most, election to major town office was the culmination of a career. The most prominent of these men entered the provincial hierarchy at the lowest level, as justices of the peace, but only in exceptional circumstances did an inhabitant of a town of this type achieve the rank of judge or councilor. For inhabitants of the suburban towns virtually the only avenue into the higher provincial elite was to be found in the nearby city, where there was potential mobility. In Dedham, for example, a son of the local minister was the only councilor, Samuel Dexter, who had made a fortune as a Boston merchant and then retired in middle age to his native town. Similarly, Thomas Westbrook Waldron, the only councilor in Dover, New Hampshire, had spent much of his youth in Portsmouth, where he had been a merchant and had served as selectman.[11]

Self-Contained Towns. A fourth type of town was the small, self-contained farming village. Examples of this type of community, found throughout the interior of the colonies, are Medfield, Boxford, Pelham, and Hampstead. In these towns, the distribution of property was more equal than in the larger towns, with one-third or less in the hands of the richest 10 per cent, and the social structure was less stratified. Since these towns contained few wealthy men, few of the town leaders could afford to devote time to town business year after year, and the major offices were shared widely. Leaders served an average of five terms or less, and those who served more than five provided less than one-half of the total service. Family continuity was not a major factor in the

[11] Lawrence Shaw Mayo, "Samuel Dexter," *Dictionary of American Biography* (New York, 1930), V, 280; Portsmouth Town Records.

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office-holding patterns. For men in towns of this type, entry into provincial office was rare. The appointment of an occasional justice of the peace was a great event, and the promotion of a justice to the quorum could be a cause for genuine consternation. When Henry Adams of Medfield heard of his pending promotion in 1773, he wrote immediately to Samuel Dexter, the Dedham councilor, to ask whether the change would disqualify him from serving as a country justice and to find out what his new duties would entail.[12] Such towns, isolated, self-contained, and governed with the participation and consent of the inhabitants, were preeminently the "Peaceable Kingdoms" that Michael Zuckerman has so ably described.

Frontier Towns. Fifth and last in the typology are the frontier towns. Properly speaking, the characteristics of these towns describe not a distinct type of town, but rather a stage of development through which all new towns passed before taking their places in one of the first four categories. Since the eighteenth century was a time of rapid settlement, however, and since a large number of towns could be found at any given time in this stage of development, it seems worthwhile to treat the frontier town as a distinct type.

During their first years, the social and political life of most new settlements were characterized by disorder and rapid change. In most new towns little stratification of property ownership had taken place, and, typically, the top 10 per cent of the taxpayers owned little more than one-quarter of the property. No clear social or political hierarchy existed, and important decisions had to be approved by all inhabitants, not merely settled among the leaders. Men served as leaders on a trial basis, some to be replaced by more able newcomers, others to stay and be remembered as the town's fathers. Under these circumstances, patterns of political behavior on the frontier were not consistent. In some towns, offices changed hands rapidly; in other towns, few candidates appeared and the same men stayed on. Regardless of their tenure, leaders in frontier towns participated little in provincial affairs. Only as the towns matured and evolved into one of the four permanent types did their political order take on consistent characteristics.

The social and political typology of towns outlined here has

[12] Dexter to Adams, Oct. 13, 1773, Adams-Morse Papers, Massachusetts Historical Society, Boston.

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clear advantages over those developed previously. Traditionally, when historians have sought to distinguish types of communities within the colonies, they have done so on a regional basis, usually by distinguishing a commercial coastal region, an established inland agricultural area, and a frontier zone to the west.[13] Such a classification system is ostensibly derived from an analysis of the economic character of each region, which then is alleged to define its social and political order. In economic terms, however, societies rarely consist of a series of homogeneous bands; more often they comprise a network of producing areas and local trading centers which in turn feed a hierarchy of larger commercial centers. This insight is most clearly expressed in *central place theory*, a theory developed by geographers to describe, in economic, political, and social terms, the relationships among the various centers of population in a geographic area.[14] The particular virtue of the typology used in this paper is that it groups towns by explicit measures of social and political behavior (and potentially by economic behavior as well), instead of relying on predictions from the apparent economic character of a region. As a result, by applying the concept of the hierarchy of places to the classification of towns, the differences among the towns of a region in size, social structure, political behavior, and elite formation become focal points for analysis instead of inconvenient aberrations, allowing fresh insights into the structural aspects of social and political behavior at both the local and provincial levels.

In addition to facilitating the interpretation of local political behavior, the typology of towns can be used to study a variety of questions ranging from the voting patterns of town representatives

[13] This arrangement of homogeneous zones, in reality a legacy of the Turnerian concern for the moving frontier, is more often implicitly assumed than explicitly developed. See, for example, Percy Wells Bidwell and John I. Falconer, *History of Agriculture in the Northern United States, 1620-1860* (New York, 1925), 69-144. Main, 7-43, and Robert M. Zemsky, "The Massachusetts Assembly" (Unpubl. Ph.D. Diss., Yale University, 1967), propose more sophisticated, but still essentially regional models.

[14] A convenient bibliography on central place theory may be found in James T. Lemon, "Urbanization and the Development of Eighteenth-Century Southeastern Pennsylvania and Adjacent Delaware," *William and Mary Quarterly*, XXIV (1967), 503n. See also G.W. Skinner, "Marketing and Social Structure in Rural China," *Journal of Asian Studies*, XXIV (1964), 3-43, and XXIV (1965), 195-228.

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on specific legislative issues to the appeal of religious movements to townspeople in different kinds of towns to the relative willingness of the towns to accept partisan politics with its inherent institutionalization of conflict in the decades after the Revolution. Such applications, of course, would require more extended treatment than is possible in these pages. I would, however, like to turn briefly to an analysis of the relationship between the political structures in the towns and the recruitment and political behavior of the provincial elites, a subject that at once enriches the typology of towns and suggests avenues by which the typology might be applied to the study of political problems.

IV

A good starting point for the study of the relationship between town politics and the behavior of provincial elites is the question of recruitment. Were provincial officials freely recruited from the ranks of town leaders, or was selection substantially governed by considerations of family prestige or inherited status? One approach to this question is to examine the careers of provincial officers and college graduates in town politics. When the age of first election to major town offices is calculated for the justices, judges, councilors, and college educated leaders included in the index of prominence, there emerges a significant contrast to the age of first election of ordinary town leaders. Whereas the overall group of town leaders in 48 towns entered their first major office at an average age of 42.6 years, the members of the provincial elite among the town leaders first served at an average age of 34.9 years, a significantly lower figure.

Since the elite group includes college graduates, one possible explanation for this disparity is that education was a decisive factor in the early advancement of elite members. But the average for the college graduates in the group alone—33.6 years—was not significantly different from that for the civil officers alone—36.2 years—and the list of civil officers contained many men with little known formal schooling. Moreover, the educational explanation becomes even more insufficient in view of Harris's finding that well over one-half of the college graduates attaining upper-class positions in colonial New England came from upper-class

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families to begin with.[15] Henry Sherburne Jr., for example, had a Harvard diploma when he was chosen selectman of Portsmouth at the age of 22, but the fact that his father was a member of the council and leader of the popular faction in New Hampshire was certainly a more important consideration.[16]

A full examination of the issues raised by the discovery of an age disparity between ordinary town leaders and those who became members of the provincial elite would be beyond the scope of this paper. For the present, I would like to point out that the town electors were able to identify future elite members at a very early stage in their careers and that the identification process involved significant considerations of family and social status, as well as educational qualifications and sheer ability. A town's leaders did not compete for provincial office on an equal basis.

The extremely unequal distribution of the residences of provincial officers among the towns reveals another aspect of the pattern of recruitment. The typology of towns includes two groups that rarely produced provincial officeholders, one group that produced a very limited number, and two groups that produced the vast majority of the leading officials. Most obviously, this suggests that opportunities for entry into the elite varied from town to town, and, more importantly, that towns of different types had very different value to members of the elite as sources of power and influence.

In the cities, members of the provincial elite could build power in one of two ways, either by relying on essentially external factors, such as royal authority and the economic resources of overseas trade or by making themselves the spokesmen for the town's interests and the shapers of its politics. Some officials, then, had little to do with the towns and their politics; but for a much larger number, local politics was an important source of power. Thomas Hutchinson, the royal governor of Massachusetts, began

[15] P. M. G. Harris, "The Social Origins of American Leaders: The Demographic Foundations," *Perspectives in American History*, III (1969), 188. The age disparity between elite members and ordinary town leaders has important parallels with the leadership structure in the legislatures, which is discussed by Robert M. Zemsky, "Power, Influence, and Status: Leadership Patterns in the Massachusetts Assembly, 1740-1755," *William and Mary Quarterly*, XXVI (1969), 502-20.

[16] Shipton, "Henry Sherburne," VIII, 490-98.

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his career at the age of 26 by securing election as a selectman of Boston at the head of a reform movement in the last years of the Elisha Cooke period in Boston politics.[17] During the next dozen years he served six terms as selectman, declining reelection in 1745, and nine terms as representative. By 1749, when his hard-money stand caused his defeat at the polls, Hutchinson had risen to speaker of the House and had established such powerful political alliances that he was promptly advanced to the council. City politics being what they were, however, town leadership more often served as a basis for opposition politics. John Hancock, for example, served 12 years as a Boston selectman; and the Henry Sherburnes of New Hampshire, father and son, built an opposition dynasty on 23 years as selectman and 29 years as representative in Portsmouth.

Equally important as power bases were the major county towns. In the rural areas, elite reputations were built on a man's ability to speak for the inhabitants of the area, and such reputations were customarily created and maintained by establishing one's authority over a major town and its hinterland. Elite families, therefore, took an active role in local politics in virtually every major county town. A revealing exception was Cambridge, where a dual elite existed, one composed of local leaders who filled both town and county offices and the other composed of absentee West Indian sugar planters, who lived in their great houses on "Tory Row" and enjoyed their honorary status as justices of the peace, but took no part in local politics. Especially important, moreover, were local politics during the formative years of the political structure in an area. When Worcester County was incorporated, for example, the Chandler family quickly transferred its activities from stagnant Woodstock to Worcester, the growing county seat. Men like William Jennison, an ordinary selectman in Sudbury, quickly joined them to parlay five years service as a selectman and representative of Worcester into a justice's commission and a seat on the Court of Pleas.[18] Similarly, when the royal government ordered the transfer of western Bristol county including the county seat to Rhode Island, the Leonard

[17] Warden, 103-26.

[18] William Lincoln, *History of Worcester, Massachusetts* (Worcester, 1837), 58, 274-78, 356-59; Shipton, "Samuel Jennison," VI, 389.

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family of Norton and their counterparts in Taunton and Dartmouth moved swiftly to transform years of service to their towns and the surrounding villages into total control of the county apparatus in the territory left to Massachusetts.[19]

In long settled areas, however, hard work in town politics may have been less crucial to the maintenance of local authority. By the eighteenth century, the leading families in Connecticut and the Connecticut valley of Massachusetts had established elaborate kinship networks which enabled them to move easily from one town to another without weakening their power. An outstanding example was the Williams family, which had spread its power base so widely that even members in minor towns like Sharon and Enfield, Connecticut, were men of importance. Even the new elites of Worcester and Bristol counties were moving toward such networks by the end of the colonial period. By the beginning of the Revolution, John Chandler of Worcester, a judge and councilor, was related by marriage to General Timothy Ruggles of Hardwick; to John Murray, the mandamus councilor of Rutland; to James Putnam of Worcester, last royal attorney-general of Massachusetts; to Timothy Paine, a councilor and son of a Bristol County judge; and to Levi Willard, a member of the leading family of Lancaster.[20]

Of debatable value as power bases were the suburban towns and secondary county towns. On the one hand, these towns were large and complex enough to require the services of justices of the peace, but, on the other hand, they were so seriously overshadowed by the more important towns that their leaders were effectively excluded from the provincial elites. Some local leaders did find their way into the elites, especially if, like Joseph Richards of Dedham and Samuel Phillips of Andover, they had college educations and family connections.[21] Occasionally, too, established elite figures would adopt suburban towns as a power base, primarily because these towns often would agree to elect such a man to the General Court, especially if he offered to turn his wages over

[19] Washburn, 360-70; Clarke, 6-36, 56-74, 279-343; Samuel H. Emery, *History of Taunton, Massachusetts* (Syracuse, 1893), 525-600.

[20] George Chandler, *Descendents of William and Annis Chandler* (Worcester, 1893), 115-35, 222-52.

[21] Shipton, "Joseph Richards," VI, 553-55 and "Samuel Phillips," IX, 431-36.

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to the town treasurer.[22] Thus Dorchester acquired the services of Nathaniel Hatch, a judge and mandamus councilor, and Hardwick those of General Timothy Ruggles. Suburban towns were also adopted by minor city figures, who displaced town leaders in the local assignments to the commissions of the peace without serving the town in any visible way. By this method Dorchester acquired the services of Jonathan Whitney, John Jones, and William Holden.[23]

The final two types of towns, the isolated village and the frontier town, were simply too small and undeveloped to serve as effective power bases. Leaders of such towns had few contacts in high places and little chance of attracting attention or exerting much influence outside the immediate neighborhood. In 1706, the voters of Enfield, settled only a few years before, recognizing this situation, asked Joseph Parsons of Springfield and Northampton to represent them before the General Court and offered, unsuccessfully, in return a share in their township.[24] More often, however, the inhabitants of these towns faced the inability of their leaders to influence anybody more directly and simply refused to send a representative. The realities of political power in these villages were a far cry from those in Boston or Portsmouth.

In the last analysis, politics and society in eighteenth-century New England cannot be described by a single model of town behavior. Towns were both large and small, wealthy and poor, consensual and deferential, elite-dominated and egalitarian. Only by looking at each town in the context of many other towns and seeking ways of classifying them according to their social and political characteristics can we make sense out of the complexities of early New England society.

[22] See, for example, Watertown Town Records (Boston, 1906), IV, 95.

[23] Whitmore, 78, 118, 126-31.

[24] Francis Olcott Allen, *History of Enfield, Connecticut* (Lancaster, Pa., 1900), I, 305.

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TABLE OF NEW ENGLAND TOWNS

Town & County		1 Town Founded	2 Population 1765-67	3 Terms per Man	4 Leaders as % of Adult Males, 1765	5 % of Property Owned by Top 10%	6 % of Terms Served by 5-Year Men	7 Prominence Index	8 Number of Judges & Councilors
MASSACHUSETTS									
Amherst	Hampshire	1759	645	4.75	39.8	24.6	63.0	6.2	0
Andover	Essex	1650	2462	7.41	24.5	25.5	69.8	6.9	1
Boston	Suffolk	1630	15520	7.77	8.4	60.4	70.8	26.2	94
Boxford	Essex	1685	851	4.16[b]	85.5	33.9	45.9	4.7	0
Braintree	Suffolk	1640	2445	8.15	25.7	59.1	69.2	11.9	2
Brookline	Suffolk	1705	338	9.67	86.3	33.3	76.8	53.3	0
Cambridge	Middlesex	1630	1582	8.26	36.5	50.1	76.5	47.4	19
Dedham	Suffolk	1636	1919	7.61	25.9	35.8	62.7	7.3	1
Fitchburg	Worcester	1764	259	6.28	63.6	26.8	58.6	0.0	0
Grafton	Worcester	1735	760	4.80	64.1	—	45.2	2.6	0
Hadley	Hampshire	1661	573[a]	6.25	33.8	35.1	71.3	14.0	2
Hardwick	Worcester	1739	1010	7.24	27.9	25.5	71.0	7.9	2
Hatfield	Hampshire	1670	815	11.32	45.1	—	86.4	18.4	4
Medfield	Suffolk	1651	639	5.58	106.1	28.5	49.4	4.7	0
Norton	Bristol	1711	1942	7.54	19.7	40.8	71.0	9.3	5
Oakham	Worcester	1759	270	4.97	86.7	34.3	62.8	0.0	0
Pelham	Hampshire	1743	371	4.31	127.9	28.5	38.1	0.0	0
Springfield	Hampshire	1638	2755	8.42	20.4	38.2	73.6	14.6	10
Sudbury	Middlesex	1639	1773	7.17	43.7	35.0	67.5	5.1	0
Swansea	Bristol	1667	1799	6.35	35.7	51.5	64.3	8.3	1
Topsfield	Essex	1650	719	7.44	73.0	—	66.1	0.0	0
Newcastle	Lincoln	1753	454	5.00	46.7	—	48.9	0.0	0

TABLE OF NEW ENGLAND TOWNS (CONT'D)

		1	2	3	4	5	6	7	8
NEW HAMPSHIRE									
Bedford	Hillsboro	1750	362	4.24	78.5	41.8	24.1	2.8	0
Concord	Rockingham	1732	752	5.92	19.3	25.2	59.0	6.7	0
Derryfield	Hillsboro	1751	230	5.14	116.7	24.4	51.4	4.4	0
Dublin	Cheshire	1771	160	5.00	60.0	26.1	55.4	0.0	0
Hampstead	Rockingham	1749	644	4.13	44.8	—	33.0	3.1	0
Hampton	Rockingham	1638	1449[a]	4.46	55.8	—	41.7	6.2	2
Londonderry	Rockingham	1722	2389	4.11	27.5	24.6	44.6	1.3	0
New Ipswich	Hillsboro	1762	600	3.67	34.7	27.4	40.0	5.0	0
Portsmouth	Rockingham	1630	4466	6.33	14.0	47.4	70.0	21.3	52
RHODE ISLAND									
Exeter	Kings	1742	1634	8.63	22.8	47.0	73.8	2.4	1
Smithfield	Providence	1731	2405	6.79	18.6	42.7	77.4	4.6	6
South Kingstown	Kings	1722	2374	7.16	26.2	56.4	79.3	14.8	20
CONNECTICUT									
Bolton	Hartford	1720	884	10.08	32.1	27.1	65.3	0.0	0
Enfield	Hartford	1695	1205	11.23	33.2	—	81.5	7.5[c]	0
New Haven	New Haven	1638	6690	5.93	16.7	27.7	58.5	18.5[c]	6
Newtown	Fairfield	1717	1741	4.99[b]	39.7	25.8	48.9	1.8[c]	0
Stonington	New London	1658	4465	7.98[b]	16.8	—	70.3	7.6[c]	1

[a] To reduce distortion, the combined populations of Hadley and its former east precinct, Amherst (incorporated as a town, 1759) have been used to calculate Hadley's figures in columns 4 and 7; and the combined populations of Hampton and North Hampton (incorporated 1742) have been used for Hampton's figures.

[b] Data on moderators not available for Boxford, Newtown, and Stonington.

[c] Prominence figures for Connecticut towns tripled to compensate for extremely low turnover of offices.

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Appendix: Sources of Data on Towns

PUBLISHED RECORDS: Edward Carpenter, *History of the Town of Amherst, Massachusetts* (Amherst, 1896), includes town records; *Reports of the Boston Records Commissioners, I-XXXI* (Boston, 1876-1904); Samuel A. Bates, ed., *Records of the Town of Braintree, 1640-1793* (Randolph, 1886); *Muddy River and Brookline Records* (Boston, 1875), I; Don Gleason Hill, ed., *Dedham Town Records* (Dedham, 1899), V; Julius Tuttle, ed., *Dedham Town Records* (Dedham, 1936), VI; Nathaniel Fisher, ed., *Dedham Town Records* (Dedham, 1969), VII; Walter A. Davis, ed., *Old Records of the Town of Fitchburg, Mass., 1764-1789* (Fitchburg, 1898), I; Henry M. Burt, *The First Century of the History of Springfield, 1636-1736* (Springfield, 1898-99), includes Town Records, 1636-1736; *Town Records of Topsfield, Massachusetts* (Topsfield, 1917-20), I-II; *Early Records of Newcastle, Maine from June 24, 1756 to January 6, 1779* (Damariscotta, 1914); *Concord* [N. H.] *Town Records, 1732-1820* (Concord, 1894); *Early Records of Derryfield*, Vols. VIII-IX of the Manchester Historical Collections (Manchester, 1905-06), I-II; *Early Records of Londonderry, Windham, and Derry, 1719-1762*, Vol. V of the Manchester Historical Collections (Manchester, 1908), I; Francis Olcott Allen, *History of Enfield, Connecticut* (Lancaster, Pa., 1900), I-III, includes town records; *Hartford Town Votes*, Vol. VI of the Collections of the Connecticut Historical Society (Hartford, 1897); Zara Jones Powers, ed., *New Haven Town Records, 1684-1769*, Vol. III of the New Haven Historical Society, Ancient Town Records Series (New Haven, 1962).

MANUSCRIPT RECORDS (at town clerk's office unless otherwise noted): Andover Town Records, 1700-85 (microfilm copy at Andover Public Library); Boxford Town Records, tax lists; Dedham Town Records, 1736-75; Hadley Town Records, 1700-85; Hatfield Town Records, 1700-85; Medfield Town Records, 1700-65; Springfield Town Records, 1736-85; Sudbury Town Records, 1700-85; Swansea Town Records, 1700-85; Bedford (N. H.) Town Records, 1770-85, at the New Hampshire Historical Society (NHHS); Dublin Town Records, 1771-1800, at NHHS; Hampstead Town Records, at NHHS; Londonderry Town Records, 1760-85, at NHHS; Portsmouth Town Records, 1700-85, WPA typescript, at New Hampshire State Library; Exeter (R. I.) Town Records, 1742-85; South Kingstown Town Records, 1723-85; Ashford (Conn.) Town Records, 1716-85 (1729-85 is also available at Connecticut State Library); Bolton Town Records, 1721-85.

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ham, Massachusetts (New Haven, c. 1947); C. O. Parmenter, *History of Pelham, Massachusetts* (Amherst, 1898); George Francis Dow, *History of Topsfield* (Topsfield, 1940); *History of Bedford New Hampshire . . . by a Committee of the Town* (Boston, 1851); John B. Clarke, *Manchester: A Brief Record of its Past* (Manchester, 1875); John Scales, *History of Dover, New Hampshire* (Dover, 1923); Levi W. Leonard, *History of Dublin, New Hampshire* (Dublin, 1919); Harriette E. Noyes, *Memorial of the Town of Hampstead, New Hampshire* (Boston, 1899); Joseph Dow, *History of the Town of Hampton* (Salem, 1893); Edward L. Parker, *History of Londonderry* (Boston, 1851); Frederick Kidder and Augustus Gould, *History of New Ipswich* (Boston, 1852); Thomas Steere, *History of the Town of Smithfield, Rhode Island* (Providence, 1881); Edward F. Atwater, ed., *History of the City of New Haven* (New York, 1887); Richard Anson Wheeler, *History of the Town of Stonington, Connecticut* (New London, 1900).

OTHER SOURCES: Clifford K. Shipton, *Sibley's Harvard Graduates* (Boston, 1933-69), IV-XV; Franklin B. Dexter, *Biographical Sketches of Graduates of Yale College* (New York, 1885), I; A. O. Austin, *Genealogical Dictionary of Rhode Island* (Baltimore, 1969); Sybil Noyes *et al.*, *Genealogical Dictionary of Maine and New Hampshire* (Portland, 1928-39); William H. Whitmore, *Massachusetts Civil Lists for the Colonial and Provincial Periods* (Albany, 1870); Jeremy Belknap, *History of New Hampshire* (Philadelphia, 1784-92), I-III; *New Hampshire: The Official Succession for Two Centuries* (Concord, 1879); *Register of New Hampshire and Almanack for 1768* (n.p., n.d.); Evarts B. Greene and Virginia Harrington, *American Population Before the Federal Census of 1790* (New York, 1932); John Russell Bartlett, ed., *Colonial Records of Rhode Island* (Providence, 1855-67), I-X; Charles J. Hoadly, ed., *Public Records of the Colony of Connecticut* (Hartford, 1868-90), IV-XV; *Acts and Resolves of the Province of the Massachusetts Bay* (Boston, 1869-1922), I-XXI; Massachusetts Archives, CXXX-CXXXIV, at the Statehouse, Boston; James A. Henretta, "Economic Development and Social Structure in Colonial Boston," *William and Mary Quarterly*, 3d. Ser., XXII (1965), 75-92.

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Labor Forces and Race Relations: A Comparative View of the Colonization of Brazil and Virginia

RICHARD R. BEEMAN
University of Pennsylvania

The adaptation of Portuguese and English political and religious institutions to new environments, the paths of land settlement, the rates of migration, and countless other social, political, geographic, and demographic differences helped contribute to the distinctive development of each of the New World colonies in Brazil and Virginia. But the initial stages of the English and Portuguese experiences, though separated in time by nearly three-quarters of a century, paralleled one another in one important, negative respect: although both Brazil and Virginia would ultimately yield handsome profits, neither colony during the first century of existence was capable of producing the instant wealth of a Cuzco or Tenochtitlán.[1] Moreover, the ways in which the Portuguese and the English initially went about making a profit from their western colonies—lands that did not immediately yield either the material or the human resources that they had anticipated—were, at least in the first two decades of those colonies' existence, strikingly similar. Both nations first entrusted the job of colonization to a relatively small band of private adventurers, and in both cases those individuals proved unable to

[1] I am concerned here primarily with the first century of colonization in Brazil and Virginia and, therefore, given the patterns of English and Portuguese settlement, will focus on Portuguese efforts in the northeast of Brazil and on English colonization on the eastern coastal plain of Virginia.

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recruit a labor force capable of turning their respective colonies into profitable enterprises. At this juncture—which coincided roughly in both colonies with the time in which private enterprise was replaced by royal control—the economic policies pursued in Brazil and Virginia began to diverge. The Portuguese were to make a series of decisions with regard to the economic organization of their colony, and in particular with regard to the recruitment of a labor force to carry out that organization, that would drastically change the character of their western colony. The English, although faced with much the same conditions the Portuguese had faced three-quarters of a century before, were, for a variety of reasons, unable to follow the Portuguese example in solving their own labor problem in Virginia. Those decisions on the recruitment of a labor force and, even more important, the timing of those decisions would ultimately help shape the differing characters of the cultures of Brazil and Virginia.[2]

The Portuguese and English Models for Colonization

The composition, motives, and expectations of the English and Portuguese colonists in America were far removed from the realities of the New World. That they were so unrealistic was almost wholly due to the lack of any comparable examples of colonization. Both Portugal and England drew on the two most familiar examples, but neither was adequate to the task of building permanent and profitable colonies in the initially hostile environment of Brazil and Virginia. The first—the launching of an overseas commercial enterprise—was based on Portugal's fantastically successful trading ventures in West Africa and India and on England's increasing interest in similar markets in India and eastern Europe. The second—a military expedition designed to exploit the existing physical and human resources of the New World as

[2] Sigmund Diamond, "From Organization to Society: Virginia in the Seventeenth Century," *American Journal of Sociology*, LXIII (1958), 457-75 and Diamond, "An Experiment in Feudalism: French Canada in the Seventeenth Century," *William and Mary Quarterly*, XVIII (1961), 1-34, has stressed the critical importance of labor supply and labor discipline in the early development of both Virginia and French Canada. This focus on economic organization can, I think, prove helpful in explaining the initial stages of social and political development, as well as economic development, in all of the North and South American colonies.

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swiftly as possible—was drawn from the astounding tales of Spain's success in the New World, tales of gold and silver and of passive natives willingly giving their labor to their European conquerors.[3] The important thing about both of these earlier ventures is that neither of them anticipated the need for a European labor force. The Europeans foresaw the need for military boldness and entrepreneurial skill, but it would be many decades before they would become reconciled to the fact that the environments of both Brazil and Virginia were not suited to a purely extractive enterprise, but rather necessitated hard, sustained labor on the part of the European colonists themselves.[4]

[3] A number of historians have suggested other possible models. Edmund S. Morgan, in an article which has helped to stimulate much of my thinking on this subject, "The Labor Problem at Jamestown, 1607-18," *American Historical Review*, LXXVI (1971), 595-611, has suggested that, in addition to the mounting of a military expedition, many Englishmen thought they were traveling to settle wastelands or to build a society resembling England. Charles M. Andrews, *The Colonial Period of American History* (New Haven, 1934), I, 53-77, describes the English enterprise as one composed of a land-hungry lower class anxious to found a colony of permanent settlement and a courageous and innovative "merchant-capitalist" class that provided the actual leadership for the venture. I would not deny that these motives were present in the minds of some, but, in my judgment, the commercial and military motives were paramount. David B. Quinn, *The Elizabethans and the Irish* (Ithaca, 1966), 106-22, argues that the English plan for the occupation of Ireland, a plan that envisioned the elimination of the Irish aristocracy and the exploitation of the Irish laboring classes, served as an important model for some of the organizers of the Virginia enterprise. Mr. Quinn's argument is a persuasive one and perhaps deserves more attention than I have given it. The Irish example does not, however, conflict with those that I have suggested. The English plan for the subjugation of Ireland, like the Virginia plan, was drawn in large part from the experience of the Spanish in Mexico and Peru. Another plausible reason for the English effort in the New World—the longstanding desire to find a passage to the Pacific—has also been given too little attention in these pages. There is no question that some prominent members of the Virginia Company, such as Sir Thomas Smith, were deeply involved in the search for a western passage. In this effort, too, commercial and military techniques were of overriding importance.

[4] In particular, the resources that Europeans were most interested in—gold, silver, copper, and precious stones—were sadly lacking. The Indians of Virginia wore some copper jewelry, but most of the copper had been obtained from other Indian cultures far to the northwest. Similarly, the Indians of Brazil possessed a few gold and silver trinkets, but these would serve only to give the Portuguese settlers a false impression of the land. Not until two centuries after the discovery of Brazil would the Portuguese finally locate any important sources of gold, and those mines were 250 miles to the west of the original settlements, in the province of Minas Gerais. Nancy Oestreich Lurie,

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Portugal had been the first European nation to embark on voyages of exploration into unknown lands and waters. Beginning in 1418, when Prince Henry the Navigator organized a voyage to Porto Santo, the Portuguese had undertaken an impressive series of explorations down the west coast of Africa. By 1487, they were headed up the east coast of Africa, and finally, in 1497, Vasco da Gama succeeded in establishing the long sought-after sea route to India.[5] Portugal used these discoveries to her commercial advantage almost immediately. By 1449, she had set up trading posts along the west coast of Africa and was trading corn, cloth, and horses for African slaves and gold. In 1505, Francesco de Almeida was granted viceregal authority over the new found lands of India. He immediately entered into diplomatic alliances aimed at securing for Portugal a monopoly over the valuable Indian spice trade. By the second decade of the sixteenth century, Alfonso de Albuquerque, with the help of Portuguese naval power, was turning Almeida's diplomatic successes into concrete, commercial gains. At the time of Albuquerque's death in 1515, the Portuguese had secured the valuable ports of Goa, Calicut, Malacca, and Ormuz and had earned lucrative commercial concessions from the rulers of western India, Ceylon, Bengal, Burma, and Siam. By their initiative in overseas expansion and by their willingness to use military power to impose their wishes on recalcitrants, the Portuguese earned profits estimated at one million cruzados per year from the Indian spice trade, with some ships realizing profits of roughly one hundred times the total cost of the voyage from Portugal to India.[6]

The East was so profitable in fact that the Portuguese were not at first particularly anxious to found colonial settlements in the West. When Pedro Alvares Cabral stumbled across the land of

"Indian Cultural Adjustment to European Civilization," in James M. Smith, ed., *Seventeenth Century America: Essays in Colonial History* (Chapel Hill, 1959), 4-46; Charles C. Willoughby, "The Virginia Indians in the Seventeenth Century," *American Anthropologist*, IX (1907), 70-73; Charles R. Boxer, *The Golden Age of Brazil, 1695-1750: Growing Pains of a Colonial Society* (Berkeley, 1962), 30-60; and João Antonil, *Cultura e Opulencia do Brasil* (Lisbon, 1711; São Paulo, 1923), 205-61.

[5] A land route to India had been discovered by the Portuguese as early as 1488. H. V. Livermore, *A History of Portugal* (Cambridge, 1947), 188-92, 220-21, 229-32.

[6] *Ibid.*, 232-38.

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Brazil in 1500, Portugal was just beginning to reap the enormous rewards of the trade with India; the land and the people that Cabral had discovered did not seem promising enough to divert Portuguese investment.[7]

Pedro Vaz Caminha, chronicler of Cabral's voyage to India via Brazil, issued a guarded prediction that pearls might be found in Brazil, but this was probably based more on his recently acquired knowledge that Christopher Columbus had found pearls during his third voyage than it was on any concrete evidence of the existence of such precious stones in Brazil. Cabral's crew did discover some small Indian villages and some apparently friendly natives. At least they appeared friendly. Unfortunately, on the seventh day of Cabral's stay in Brazil, a group of Indians, their faces beaming with friendly smiles, proceeded to beat on the head with clubs, and then eat, a member of Cabral's advance scouting party. The whole scene occurred in full view of the ship's crew.[8] This would prove to be only the first of many dispiriting encounters between the Portuguese and the aboriginal inhabitants of Brazil. Although not all of the Indians of Brazil proved consistently hostile to the Portuguese settlers, their behavior toward outsiders was erratic enough to make them less than ideal partners in the building of a new colony.[9]

[7] There has been considerable debate among historians as to whether Cabral discovered Brazil by accident or whether he was consciously planning to stake out a Portuguese claim in the New World. For a review of the historiographical problem and a careful statement of the facts pointing to the conclusion that Cabral's landing was accidental, see W. B. Greenlee, ed., *The Voyage of Pedro Alvares Cabral to Brazil and India* (London, 1937), xlvi-lxvii.

[8] Letter of Pedro Vaz de Caminha to King Manuel, May 1, 1500, *ibid.*, 9; Robert Southey, *History of Brazil* (London, 1810), I, 15.

[9] The most numerous and powerful Indian culture of northeastern Brazil, the Tupí-Guaraní, was an extremely loose confederation of separate tribal units bound together only by a common linguistic stock. Considerable data have been gathered on the Tupí-Guaraní communities, but the most important fact for this study is that the members of nearly every Tupí-Guaraní village literally detested the members of every other Tupí-Guaraní village, with each village carrying out interminable warfare against the others. This intense hostility among the Indian tribes was the product both of a struggle for political supremacy and of the desire for captives with which to satisfy their ritualistic needs for human sacrifices. Robert H. Lowie, "The Tropical Forest Tribes," in Julian Steward, ed., *Handbook of South American Indians* (Washington, D. C., 1948), III, 1-47; and Alfred Metraux, "The Tupinamba," in Steward, III, 97-116.

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Cabral discovered the brazilwood tree—the source of an important dyestuff for the European textile industry—during his brief stay in the land he called Terra do Vera Cruz, but the reddish dye obtained from the brazilwood could hardly justify a major investment in the hostile environment of the New World when the silks and spices of India yielded superior returns.[10] When Cabral sailed from Brazil he left behind two convicts. The two unfortunates immediately "began to weep, and the men of the land comforted them and showed that they pitied them."[11] A few decades later when the Portuguese sovereign compared the magnificence of New Spain and Peru with the unpromising character of his possession in the New World, there would be still more cause for weeping.

The Portuguese crown, because it was interested in exploitation and not in colonization, waited over three decades, until 1533, before drawing up a plan of settlement for Brazil. This 33-year hiatus is roughly comparable to the period separating England's initial experiment with colonization in 1584 from the permanent settlement of Jamestown in 1607, although Portugal was slightly more active in the interim in Brazil than were the English in North America.

The Portuguese, through necessity, took some rather rudimentary steps to profit from the brazilwood tree and to safeguard their discovery from foreign interlopers. In 1502, the crown awarded a contract to Fernão do Loronha, a converted Jew from Lisbon, to trade in brazilwood. In 1516, Christavão Jacques was appointed captain of the royal coast guard in an attempt to put an end to French incursions into the territory. By 1533, the Portuguese had established at least two trading stations along the northeastern coast, and it had been proven that the brazilwood trade was at least marginally profitable.[12] Nevertheless, the Por-

[10] The brazilwood tree, although no substitute for gold and other precious metals, was sufficiently valuable that the Portuguese named their new western colony after it. Other than that one commodity, however, Portugal's new colony in America seemed distinctly unpromising. The Indian economy of northeastern Brazil was confined to the cultivation of manioc, a coarse grain, and to hunting and gathering. Alexander Marchant, *From Barter to Slavery: The Economic Relations of Portuguese and Indians in the Settlement of Brazil, 1500-1580* (Baltimore, 1942), 28-33.

[11] "Anonymous Narrative," in Greenlee, ed., *Cabral's Voyage*, 60.

[12] Marchant, *Barter to Slavery*, 28-47.

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guese colony, if indeed it deserved to be called a colony, was not a thriving enterprise. Over three decades after its discovery, Brazil was only sparsely inhabited by Europeans, and Portuguese control of the territory existed only in the legalistic phrases of the Treaty of Tordesillas. The Portuguese, at least partially because of their preoccupation with the riches of the East, were unwilling to supply the men or material necessary to eject French intruders from the area, and, as a result, no one could claim to have practical control over the territory of Brazil.

By 1530, Portugal began for the first time to think of establishing permanent settlements in Brazil. This was not a sign that she had given up the hope of a colony of commercial and military exploitation, but merely an indication that external pressures had forced the crown to look for new areas to exploit. First, the India trade was languishing; by 1530, the price of spices had dropped precipitously, a result of oversupply. The court at Lisbon, which had lived extravagantly on the revenues from the spice trade, discovered that the people of the world were disposed to consume only so much pepper. Second, both France and Spain were threatening to shut out the Portuguese from the New World. Spain, after her spectacular success in Mexico, was beginning to expand in every direction in the western hemisphere, and the French, in 1530, had succeeded in destroying one of Portugal's largest brazilwood trading posts in Pernambuco.[13]

Finally, when Cabral first happened on Brazilian shores in 1500, Spanish accomplishments in the New World were limited to three relatively unprofitable voyages by Christopher Columbus, a few exploratory voyages near the coast of Venezuela, and a relatively unstable colony on the island of Santo Domingo. By 1530, when Portugal began her plans for the permanent settlement of Brazil, reports of Cortes's success in Tenochtitlán had spread to Portugal. In 1533, the year the Portuguese actually began to colonize Brazil, Francisco Pizarro was extracting entire rooms filled with gold and silver from the captive Inca Prince Atahualpa. It was a cruel irony; Portugal had pioneered in the

[13] W. B. Greenlee, "The First Half Century of Brazilian History," *Mid-America*, XXV (1943), 91-120, gives a much more optimistic view of the Portuguese accomplishments in Brazil during this period, but he admits that, by 1530, there was an increasing need to strengthen Portuguese control over the area.

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arts of navigation and exploration and had even employed Columbus at one time. Now she had to stand by and watch Spain reap the rewards. But there was still hope. Spain had invested more time and money in her western ventures than had Portugal and had not achieved substantial returns until she had penetrated into the interior. Although there was little in the Brazilian landscape that recommended it as a source of easy wealth, the Portuguese were not ready to abandon the notion that either the land or the people would eventually yield at least some of the same riches as New Spain and Peru.

The English were, if anything, even more hungry than the Portuguese for commercial and military gain in the New World, for they had entered the game of overseas expansion late. By 1555, however, they had begun to make minor inroads into markets that had long been dominated by the Venetians, Spanish, and Portuguese. Both the Muscovy Company (1555) and the Levant Company (1579) represented efforts by groups of private individuals with royal approval and occasional royal backing to challege other nations for supremacy in the eastern overseas markets.[14] But prior to the seventeenth century, the English had no overseas colonies; nor had they succeeded in gaining a strong foothold in any of the major foreign markets. Despite the "progressive," "capitalistic" spirit of the English trading companies, English overseas enterprise in the sixteenth century was a failure.[15]

And the English publicists knew it. Beginning in 1553, when Richard Eden became England's first defender of "imperialism," and culminating in the promotional tracts of the Virginia Company of London, English propagandists incessantly urged their countrymen to launch an imperialistic and exploitative enterprise before the Spanish were able to shut them off completely from the lucre to be found in the New World.[16] Most of the propagandists

[14] Andrews, I, 31-37.

[15] Charles Lucas, *The Beginnings of English Overseas Enterprise* (Oxford, 1917), 143-55 and Andrews, I, 43, claim that the joint stock company "laid the foundations of England's naval, maritime and commercial greatness," while at the same time paving the way for English democracy. Their assertion hardly seems tenable in light of the time lag of at least a century between the establishment of the joint stock companies and the bare beginnings of English success.

[16] An excellent introduction to the propaganda literature of English ex-

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would have preferred, if possible, to obtain this wealth through peaceful commerce, but like the Portuguese in India and the Spanish in the New World, they were willing to use military means if necessary.

Richard Eden in his translation of Sebastian Munster's *Cosmographia* pleaded with his countrymen to take advantage of the economic and commercial possibilities of colonization and berated those who shied away from overseas expansion out of fear of the Spanish.[17] In his translation of Peter Martyr's *Decades of the Newe World* (1555), Eden became even more vocal in his advocacy of English imperialism. Although it was fashionable in England to criticize Spain for her excesses in the New World, Eden bluntly told his English readers to spend less time denouncing Spain and more time following her example. Spain, after all, was the acknowledged leader in overseas expansion; it was necessary to "stoope England stoope, and learne to knowe thy lorde and master, as horses and other brute beasts are taught to doo."[18]

Thomas Nicholas, in his translation and popularization of a section of Lopez de Gómara's biography of Hernan Cortes (1578), went even further than Eden in opening his reader's eyes to the undiscovered riches of the New World. Although Gómara's book was in effect a history of the Spanish conquest of Mexico, Nicholas in his "Epistle Dedicatory" offered it as a practical guidebook to the successful colonization of North America:

> Here shall they see the wisdome, curtesie, valour, and pollicie of worthy Captynes, yea, and the faithful hartes whiche they ought to beare unto their Princes services; here also is described how to use and correct the stubborn & mutinous persons, & in what order to exalt the good, stoute & vertuous souldiers, and chiefly, how to preserve and keepe that bewtifull Dame Lady Victorie when she is obtayned.[19]

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pansion, and one which I have profited from enormously, is John Parker, *Books to Build an Empire: A Bibliographical History of English Overseas Interests to 1620* (Amsterdam, 1965).

[17] Richard Eden, *A Treatyse of the Newe India . . .*, in Edward Arber, ed., *The First Three English Books on America* (Westminster, 1895), 7-11.

[18] Richard Eden, *The Decades of the Newe World . . .*, in Arber, 49-60, esp. 52.

[19] Lopez de Gómara, *The Conquest of the Weast India*, trans. T. Nicholas (London, 1578; New York, 1940), aii.

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For Nicholas, "Dame Lady Victorie" meant gold, and he was quick to assure his readers that the English, if they persevered, would find just as much gold as the Spanish. In support of this contention, he announced:

> where it was supposed that the golden metall had his beginning in the East and Weast India, neare unto the hot zoane, it is nowe approued by the venterous travellour and worthy captayne Martin Frobisher Esquire . . . that the same golden metall dothe also lie incorporate in the bowelles of the Northeaste parties.

Thus, with hopes raised by the early and erroneous reports of the Frobisher voyage, Nicholas was able to promise the English rich rewards if only they would invest in an overseas colonizing venture.[20]

These rhapsodic descriptions of the promise of the New World continued even after the English had made sustained contact with North America. Arthur Barlowe's description of the North Carolina coast, made during Sir Walter Raleigh's first voyage and later published in Richard Hakluyt's *The Principall Navigations, Voiages and Discoveries of the English Nation* is one of uncritical praise. Barlowe found the people to be "the most gentle, loving, and faithfull, void of all guile and treason, and such as lived after the manner of the golden age." The Indian women fed the English, entertained them, and even washed their feet. The men would trade fifty animal skins, worth at least fifty crowns, for one copper kettle. And, furthermore, these submissive natives lived on a land which "bringeth forth all things in abundance, as in the first creation, without toil or labor."[21]

The vision of North America as a land of wealth and effortless exploitation reached its most popular and extreme form in 1605 when George Chapman, Ben Jonson, and John Marston wrote the English comedy, *Eastward Ho!* The scene is an English tavern, the participants are perhaps a trifle loose tongued because of the wine, but their description of Virginia, even when one makes allowances for exaggeration, is revealing:

> *Seagull:* A whole countrie of English is there, man, bread of those that were left there in '79; they have married with the Indians, and

[20] *Ibid.*

[21] Richard Hakluyt, *The Principall Navigations, Voiages and Discoveries of the English Nation* (London, 1589), 729-33.

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make 'hem bring forth as beautiful faces as we have in England; and therefore the Indians are so in love with 'hem that all the treasure they have they lay at their feete.

Scapethrift: But is there such treasure there, Captain, as I have heard?

Seagull: I tell thee, golde is more plentiful there than copper is with us. . . . Why, man, all their dripping pans and their chamber-pots are pure golde; and all the chaines with which they chaine up their streets are massie golde; all the prisoners they take are fettered in golde; and for rubies and diamonds they goe forthe on holy dayes and gather 'hem by the sea shore to hang on their childrens coats. . . .[22]

Doubtless no one in the audience took at face value the description given by Seagull, but that the authors would bother even to use the dialogue is evidence that similar stories, although perhaps slightly less spectacular, were already in circulation.[23]

Even Thomas Hariot and the two Richard Hakluyts, who are generally described as having been more interested in permanent, peaceful settlement than in plunder, could not free themselves from the notion that instant wealth would be waiting for the English settlers in the New World.[24] Richard Hakluyt the Elder placed primary emphasis on the commercial-imperialist motive for colonization. After two short, perfunctory sentences indicating that he favored the spread of the Christian gospel to North America, Hakluyt spent the greater part of his essay demonstrat-

[22] Ben Jonson, George Chapman, and John Marston, *Eastward Ho!* (London, 1605; Boston, 1905), 71-72.

[23] Wesley F. Craven, *The Southern Colonies in the Seventeenth Century, 1607-1689* (Baton Rouge, 1949), 70n., maintains that the play, because it treats the wealth of Virginia in such an exaggerated manner, is actually indicative of the prevailing skepticism in England regarding the riches of the New World. Skepticism there was, but that the scene was presented at all is evidence that some people in England, however misguidedly, believed that America was a land of enormous wealth.

[24] Parker, 104-05, 109, 112; E. G. R. Taylor, *The Original Writings and Correspondence of the Two Richard Hakluyts* (London, 1935), I, 1-66, stresses the fact that the Hakluyts spent considerably more time than their contemporaries in planning for a permanent, productive European settlement in the New World. The Hakluyts also had doubts about the wisdom of relying too heavily upon Indian labor and, in fact, recommended sending a sizeable English labor force to the New World. They shared the optimism of their contemporaries, however, as to the ease with which the existing wealth of the Indians could be exploited.

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ing the "possibility of the enlarging of the dominions of the Queen's Most Excellent Majesty, and consequently of her honour, revenues, and of her power by this enterprise." New colonies would provide markets for English woolens, "especially those of the coarsest sorts." In exchange for these reject woolen articles, England could purchase oil, wines, hops, and "most or all the commodities that we receive from the best parts of Europe." In the process of doing this, Hakluyt claimed, the English nation could also finally break the Spanish domination of the western overseas trade.[25]

The area eventually chosen by the English for their initial settlement, the James River basin, was populated by roughly 2500 members of the Powhatan Confederacy. Like the Indians of Brazil, they lived in many small, distinct, loosely supervised villages, an extremely inefficient mode of social and political organization from the standpoint of those who wished to exploit Indian wealth and labor for their own purposes.[26] While the residents of the various villages were not always hostile and warlike, their attitudes toward the English would prove to be sufficiently unfriendly to make them undependable partners in any commercial venture. And most important, the subsistence economy of the Indians of Virginia would hardly send the stock of English woolen mills soaring, nor was it capable of providing the English with the goods which they were at present purchasing in Europe. But this, after all, was not important. The dream of commercial empire, particularly the hope of making a dent in the overseas empire of the much hated and much envied Spain, was enough to make even a normally sensible man like Richard Hakluyt the Elder forget the harsh realities of the Virginia wilderness.

Even after observing at first hand the primitive state of the Indians on Roanoke Island, Thomas Hariot was able to maintain

[25] Richard Hakluyt the Elder, *Inducements to the Liking of the Voyage Towards Virginia* . . . (1585), in Taylor, II, 327-38.

[26] The total population of the Powhatan Confederacy, which spread throughout the Tidewater and Eastern Shore area, was approximately 8500. James Mooney, "The Powhatan Confederacy, Past and Present," *American Anthropologist*, IX (1907), 129-30, obtains these figures by evaluating the estimates of John Smith in Edward Arber, ed., *Travels and Works of Captain John Smith* (2d ed.; Edinburgh, 1910), I, 347-51; and William Strachey, *Historie of Travell into Virginia Britania* (London, 1612; London, 1953), 56-62.

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and, indeed, bolster his optimism about America. Because the natives suffered from a "want of skill and judgment in the knowledge and use of our things," it seemed all the more "probable that they should desire our friendships and love and have the greater respect for pleasing and obeying us."[27] Richard Hakluyt, the cousin of Richard Hakluyt the Elder, even formulated contingency plans in case the natives did not turn out to be as friendly as those described by Hariot. Hakluyt suggested that the English locate their settlement at the mouth of a navigable river in order that the settlers could play off one Indian tribe against the other. They could turn to tribes located inland to defend themselves against hostile Indians on the coast, or should the inland tribes be hostile, they could ally themselves with the coastal tribes. Moreover, the Indians might even be used as allies against a more dangerous enemy, the Spanish, who had "exercised moste outrageous and more than Turkish cruelties in all the Weste Indies." The Spanish were so detested by the Indians that the English could almost surely expect Indian aid in throwing off their "most intollerable yoke."[28] Thus, if the English settlers used care in taking advantage of the existing rivalries among Indian nations and between the Indians and the Spanish and if they exploited their natural advantages over the Indians, then the natives could be made to serve the English in their quest for riches in the New World.

What we find then in the English propaganda tracts is a description of a land bountiful in exotic and valuable native produce, a land blessed with a usually docile and dependable native labor supply, and a land where precious metals might perhaps be found. Given this image of America, it is not surprising that there was much talk in seventeenth-century England of limiting Spanish expansion in the New World, of gold and precious metals, of establishing a lucrative trade with the Indians, and of persuading the Indians to do the labor necessary to build a colony for the English. Nor is it surprising that little attention was paid to the

[27] Thomas Hariot, *A Brief and True Report of the New-found Land of Virginia* (London, 1588), in David B. Quinn, *The Roanoke Voyages, 1584-1590* (London, 1955), I, 371-72.

[28] Richard Hakluyt, *Discourse on Western Planting*, in Taylor, II, 246-49, 257-65.

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planning of an economy which might support a permanent English settlement primarily dependent upon its own labor.[29]

England's initial attitude toward colonization was thus not markedly different from that of Portugal. Each nation, because of its commercial experience in the East and because of the Spanish example in the West, geared its colonial policy toward rapid exploitation of existing wealth rather than the slower and more difficult task of planting new sources of wealth. Some envisioned simple commercial exploitation on the model of Portugal's success in India, but others had an even grander vision, a vision of gold mines and free Indian labor. Almost no one in either Portugal or England envisioned the kind of settlement that was most necessary for survival in the New World—a settlement where hard, sustained work by the European colonizers would be vital for existence.

The First Stages of Portuguese and English Colonization

The royal governments of both Portugal and England were at first content to let others bear the risks of colonization. In Brazil, the Portuguese crown, lacking the funds to launch a highly centralized, state-sponsored colonizing effort, parceled out grants of land to twelve private citizens.[30] These individuals, called *donatarios*, tended to be either military officers or members of the lesser nobility. Many of them had seen military service in India and Africa and thus were well acquainted with the ways in

[29] For additional evidence see Sir George Peckham, *A True Reporte of the Late Discoueries of the New-found Landes* (London, 1583); Augustin de Zarate, *The Discouerie and Conquest of the Provinces of Peru*, trans. T. Nicholas (London, 1581); Ralph Lane to Richard Hakluyt, 1585, in Taylor, II, 346-47; Henry Roberts, *A Most Friendly Farewell . . . to the Right Worshipfull Sir Francis Drake* (London, 1585); and Thomas Greepe, *The True and Perfecte Newes of the Woorthy and Valiaunt Exploytes, Performed and doone by . . . Syr Francis Drake* (London, 1587).

[30] The best accounts of this initial period of settlement in Brazil are Marchant, *Barter to Slavery*, 48-80; Pedro de Azevedo, "Os Primeiros Donatarios," in Carlos Malheiro Dias, ed., *Historia de Colonização Portuguesa do Brasil* (Oporto, 1923), III, 189-216; Roberto Simonsen, *Historia Economica do Brasil, 1500-1820* (Rio de Janeiro, 1937), I, 117-42; and Francisco Adolpho de Varnhagen, *Historia Geral do Brasil Antes da Sua Separação e Independencia de Portugal* (3d ed.; São Paulo, 1927), I, 180-290.

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which foreign lands and peoples could be exploited.[31] Unfortunately, most of them knew nothing about the requirements of building a permanent colony based on European labor.

Although there has been much debate over whether these grants were "feudal" or "capitalistic," two of the most knowledgeable students of this period of Brazilian history, Roberto Simonsen and Alexander Marchant, have concluded that while the *donatarios* possessed political and military power which appears to be feudal in origin, the actual exercise of those powers coupled with the plainly economic motive behind the granting of them made the *donatario* system roughly similar in purpose to joint stock enterprises such as the Virginia Company. Marchant argues that feudalism implies a contract between the king and his subjects in which the king grants land in exchange for military services, while Simonsen further notes that feudalism entails a system of fixed social classes.[32] Neither of these conditions was present in Brazil. The *donatarios* themselves came from different classes and the settlers living on the *donatario* grants had ample opportunity to move up and down the social scale as their economic progress dictated. Most important, the principal duty of the *donatario* to the king and of the settlers to each individual *donatario* was to exploit the land for economic gain rather than to perform military services. To be sure, the *donatario* was to provide for the defense of his land, but only as a means of fulfilling his primary economic tasks. This is distinctly nonfeudal.[33]

The *donatarios* were granted considerable authority. They were empowered to found towns, appoint all municipal officials, levy taxes on the settlers, and even to mete out capital punishment in the case of certain stipulated crimes.[34] But the powers granted to the leaders of the Virginia Company over the settlers at Jamestown were hardly inferior. By the Charter of 1606, an appointed resident council of thirteen members was to serve as

[31] Varnhagen, I, 168-74 gives a capsule biography of each of the *donatarios*.

[32] Alexander Marchant, "Feudal and Capitalistic Elements in the Portuguese Settlement of Brazil," *Hispanic American Historical Review*, XXII (1942), 493-512; and Simonsen, I, 124-27.

[33] Marchant, "Feudal and Capitalistic Elements," 500-01. For an opposing view, see Carlos Malheiro Dias, "O Regimen Feudal das Donatarias," in Dias, ed., *Historia de Colonização*, III, 219-83.

[34] Marchant, *Barter to Slavery*, 58-60.

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the sole governing board of the Virginia settlement. Ultimate political control over the colony was left in the hands of the king, just as it was in Brazil, but the wide range of controls necessary for the settlement, operation, and maintenance of the colony was left completely in the hands of the resident council.[35] It was authorized to settle new towns, to inflict summary punishment to anyone slandering the company or the resident council, to regulate hours of work, to ensure religious orthodoxy, and to conscript any and all settlers into military service.[36]

The initial leaders of the Jamestown experiment also had backgrounds similar to those of the group of petty nobles and military men who made up the Portuguese leadership in Brazil. The Virginia Company itself was well stocked with members of the nobility—over one-third of the total peerage had invested in the company at one time or another.[37] Those who played the most active role in the company's affairs were either wealthy merchants with considerable experience in the India trade or investors, such as the Earl of Warwick, in Elizabethan piracy ventures.[38] Their experience, like that of their Brazilian counterparts, suited them more to commercial and military exploitation than to the founding of an agrarian-based colony.

Those who participated in the first colonizing expeditions to Brazil and Virginia made it abundantly clear that they were not themselves prepared for difficult manual labor in their new environment. Most of the Portuguese *donatarios* showed a decided reluctance to exert the energy necessary to provide for the exigencies of life. This remained the case even after it became apparent that the land would not yield immediate profits in precious metals and exotic native produce. Those Portuguese settlers who achieved at least minimal success relied on Indians to provide them with

[35] Theoretically the *donatarios* had nearly unlimited powers, but the fact that the king, in 1549, was able to revoke the grants with no difficulty is proof that the crown, not the *donatarios*, possessed ultimate political authority over the lands.

[36] The Charter of April 10, 1606 and the instructions from the privy council to the leaders of the enterprise in November 1606 can be found in Alexander Brown, *The Genesis of the United States* (Boston, 1890), I, 46-75.

[37] Lawrence Stone, *The Crisis of the Aristocracy, 1588-1641* (New York, 1965), 372.

[38] Wesley F. Craven, *Dissolution of the Virginia Company: The Failure of a Colonial Experiment* (New York, 1932), 25-26.

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food and labor to cut their brazilwood. They used a system of barter and of temporary alliances with one village against other, more hostile villages as inducements to make the Indians work for them. The system was successful at first. In return for a few relatively worthless trinkets, the Portuguese were able to obtain a significant amount of Indian labor—an amount sufficient to keep their modest colonial enterprise going. They were able not only to begin to profit from the brazilwood trade, but also to make a tentative beginning, again with the help of Indian labor, in the cultivation of sugar.[39]

Unfortunately, the system soon started to break down. The Indians quickly tired of the novelty of the Portuguese trinkets and began to demand more valuable merchandise. As competition for Indian labor increased among the settlers, the price in barter was bid up and the Portuguese found that their production costs were skyrocketing.[40] To make matters worse, the Indians of Brazil had the annoying habit of turning against their Portuguese neighbors, occasionally being so impolite as to eat them. As Francisco Adolpho de Varnhagen has pointed out in his classic multivolume *Historia Geral do Brasil*, the civilizations and life styles of the Portuguese and Indians were so different that the members of the two cultures soon developed for one another a hearty contempt, based on mutual jealousy and ignorance.[41] Moreover, the French were often in the vicinity, ready to exploit any latent hostility between the two groups.[42] Whatever the reasons for the increasing friction between the Portuguese and the Indians, it became clear after less than two decades of settlement that the Portuguese could not depend on the Indians to enter into voluntary working arrangements on a permanent and profitable basis.

The English settlers at Jamestown, as Edmund Morgan has convincingly demonstrated, were even less anxious than the Portuguese to settle down to the serious task of building a permanent settlement.[43] Instead of tending to the crucial business of gathering supplies, growing food, and building shelter, the English

[39] Marchant, *Barter to Slavery*, 63-78.

[40] *Ibid.*, 69-80.

[41] Varnhagen, I, 276-77. Unfortunately, Varnhagen shared the early Portuguese settlers' distaste for the Indians, a bias which is reflected in his history.

[42] Marchant, *Barter to Slavery*, 73, 75-78.

[43] Morgan, "Labor Problem."

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wasted their time quarreling among themselves, looking for gold, or searching for a passage to the Pacific Ocean. When Captain John Smith left the colony in October 1609, the settlers lost what little discipline they had previously had and, neglecting even the minimal tasks necessary for survival, they ended up dying in overwhelming numbers. Discipline improved slightly with the arrival of Sir Thomas Gates and Lord De La Warr in 1610, but when Sir Thomas Dale arrived in Jamestown in 1611, he found the settlers at work at "their daily and usuall workes, bowling in the streetes."[44]

This chronic "underemployment" at Jamestown has been attributed to many factors. The surplus of derelicts and idle noblemen among the initial settlers, the lack of individual incentive in the communally oriented plan of settlement, and the low quality of work habits prevalent throughout England at the time—all offer at least a partial explanation for the early Virginians' behavior.[45] But most important, the motives behind the settlement, stated in the tracts of the English imperialist-propagandists, could not have prepared them to undertake hard, sustained work.

The English in Virginia were unwilling to abandon their hopes of finding easy wealth even after it became clear to them that Virginia was not an El Dorado. They tried continually to use Powhatan as a "front-man" for their operation—in much the same way that Cortes used Montezuma in Mexico—but the Virginia settlers soon realized that Powhatan was not so overwhelmed by the "superiority" of English civilization that he would become their servant.[46] In fact, Powhatan made a point of staying away

[44] *Ibid.*; Ralph Hamor, *A True Discourse of the Present State of Virginia* (London, 1615; Richmond, 1957), 26.

[45] Diamond, "Organization to Society," 457-75, suggests that in order to make the English settlers work, the Virginia Company continually had to increase individual incentive by loosening the corporate restraints upon them. By doing so the company did indeed increase incentive, but also experienced a corresponding reduction in the amount of discipline it could exert over the settlers. Morgan, "Labor Problem," 602-06 cites D. C. Coleman, "Labor in the English Economy of the Seventeenth Century," *Economic History Review*, 2d ser., VIII (1956), 280-95, to prove that the work habits that the lower-class English settlers brought with them to Virginia were not of the most rigorous nature. Stone, 331, maintains that "the rich and the well-born were idle almost by definition." This would presumably also apply to those who made the trip to Virginia.

[46] Powhatan, after grudgingly journeying to Jamestown for a "coronation"

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from the English; thus, they lacked even the minimal advantages of the settlers of Brazil. At least in Brazil the Portuguese were able to achieve a temporary solution to their labor problem by bartering with the Indians. The Virginia colonists, because of Powhatan's determination to remain independent of all foreign "intruders," did not even succeed in developing a dependable system of trade with the Indians.[47]

A few Virginians, even after repeated failures to exploit the Indians, remained sanguine about the possibilities of eventual success. John Martin, a member of the ruling council in Virginia, was still ruminating about the advantages of Indian labor even after the dreadful massacre of 1622. Like many other Virginians, Martin no longer viewed the Indians as gentle, docile creatures, but he nevertheless thought that it would be more profitable for the English to put them to work than it would be to exterminate them. One of the ways to persuade them to work, according to Martin, was to make them much more dependent upon the English for their safety and survival. This could be achieved, he claimed, by "haueinge some 200 Souldiers on foote, contynuallie harrowinge and burneinge all their townes in wynter and spoileinge their weares."[48]

Martin was fast becoming an exception. By 1618 at the latest, most Englishmen realized that their dream of an El Dorado in Tidewater Virginia would never become a reality. To be sure, by 1614, Virginians had found that tobacco might be a potentially profitable export commodity, but many royal officials were opposed to the cultivation of a crop that was "loathsome to the eye, hateful to the nose, harmful to the brain, dangerous to the lungs, and in the black stinking fumes thereof, nearest resembling the horrible Stygian smoke of the pit that is bottomless."[49] Furthermore, the English settlers in Virginia were lacking the crucial

ceremony, told the English that he was every bit as exalted and powerful as the king of England and that he had no intention of being duped into serving the English settlers; Arber, ed., *John Smith*, I, 124.

[47] Lurie, 46-50.

[48] Susan M. Kingsbury, ed., *The Records of the Virginia Company of London* (Washington, D. C., 1933), III, 704-06.

[49] James I, *Counter-blast to Tobacco* (London, 1604), quoted in Jerome E. Brooks, *The Mighty Leaf, Tobacco Through the Centuries* (Boston, 1952), 71.

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supply of labor needed to cultivate the "joviall weed"; the Indians had made it clear that they were not willing to serve the English, and those Englishmen who had settled in Virginia were too few, too specialized in their skills, and too lazy in their work habits to do it themselves.[50] The Virginia Company did everything possible to get the settlement off the ground. It offered inducements of free land to potential settlers who could pay their own passage, allowed settlers already in Virginia to increase the size of their holdings in order to increase individual incentive, instituted a more representative system of government, and encouraged the growth of food crops in order to prevent recurrence of the famines that had plagued the settlement. But nothing seemed to work.[51] In 1624, at the demise of the Virginia Company, the population of the colony stood at 1275 Englishmen, not much more than the population of the settlement in 1609.[52]

Clearly, something had to be done in both Brazil and Virginia to bolster the sagging fortunes of the two colonizing ventures. In each case private control over the colony was suspended and royal control introduced. In Brazil, in 1549, the *donatarios* were allowed to keep as much land as they had actually brought under cultivation, but much of their political power was taken over by a royal governor, a chief justice, and a treasury official.[53] In Virginia, the English crown took over the functions of the Virginia Company and became the sole proprietor of the enterprise. Although this action has frequently been interpreted in political terms, it is apparent that the considerations that moved the crown to dissolve the company were economic.[54] Neither the *donatarios* nor the

[50] At a time when the Virginia Company needed men to grow corn to keep the settlers alive it was advertising for such specialized skills as iron miners, cutlers, armorers, net makers, upholsterers of feathers, leather dressers, spinners of pack thread, mat makers, saltpeter men, tile makers, etc.; Brown, *Genesis*, I, 469-70.

[51] Craven, *Dissolution*, 47-79.

[52] W. Noel Sainsbury, ed., *Calendar of State Papers, Colonial Series, 1574-1660* (London, 1860), 43, 57.

[53] Varnhagen, I, 291-95.

[54] The debate over whether the crown acted justly when it dissolved the Virginia Company has raged continuously since 1624. Alexander Brown in his *Genesis of the United States* and later in *The First Republic in America* (Boston, 1898) attempts to prove that the king, acting out of a tyrannical desire to destroy the representative assembly that the company had instituted in Virginia, revoked the company's charter in order to stifle the bur-

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officials of the Virginia Company had been successful in devising a system of labor capable of producing the kinds of profits that the Portuguese and English governments had anticipated. It was only logical that the sovereigns of the two European nations would decide on a change of management.[55]

Royal Rule in Brazil and Virginia

The introduction of royal government—after sixteen years of *donatario* rule in Brazil and seventeen years of company rule in Virginia—marks the point at which the histories of the Portuguese and English settlements in the New World begin to diverge drastically. As the royal governments of Portugal and England began to plan the future of their new, overseas responsibilities, the institutions and cultural and technological influences of the two nations made their first meaningful impact on the development of Brazil and Virginia. In some cases all that occurred was the formalization and legitimization of preexisting Portuguese and English practices in the two colonies; in other cases, Portuguese and English institutions were imposed for the first time on the frontier conditions of the colonies.

The single most important decision made by the Portuguese royal government was to continue to attempt to exploit the land and the people of the New World to the fullest extent possible. Only the emphasis had changed; the exploitation was no longer to be purely extractive, but, rather, was to be aimed at utilizing Indian and imported labor in the intensive cultivation of export crops.[56] This decision caused Tóme de Sousa, the first royal governor of Brazil, to scrap his initial plans for a humane Indian poli-

geoning democratic spirit of Virginia. Craven, *Dissolution*, has demonstrated that the crown's antipathy to the political forms of the settlement was decidedly secondary to its dissatisfaction with the economic inefficiency of the company.

[55] In Brazil a consolidation of government into the hands of the Portuguese monarch was made all the more necessary because of the increasing threat posed by French interlopers in the New World.

[56] This decision seems to have been a conscious one. King João III called in his most trusted advisers and specifically asked them how, given the lack of precious metals, to best exploit the land of Brazil. Celso Furtado, *Economic Growth of Brazil* (Berkeley, 1963), 5-11; Sergio Buarque de Holanda, *Raizes do Brasil* (Rio de Janeiro, 1936), 19-139.

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cy for the colony. As the barter system broke down, the Portuguese settlers turned to outright enslavement of the Indians. Although both King João III and the royal governor disapproved of this enslavement, they were swayed considerably by the colonists' argument that with adequate labor Brazil could become one of the world's leading sugar producers. The resulting decision was a compromise. Those Indians who had demonstrated that they were hostile to the Portuguese were fair game for enslavement; those that remained at peace with the Portuguese were to be protected.[57]

Indian slavery in Brazil continued in practice, although not in law, for several centuries, but it was not in itself an adequate solution to the colony's labor shortage.[58] A number of influential people, most notably the members of the Company of Jesus, were offended by the enslavement of Indians; moreover, the Indian captives of the "just wars" were too few and often too rebellious to constitute the sole source of labor for the Brazilian colonial economy. As a result, the Portuguese turned, almost immediately after the institution of royal government in 1549, to the large-scale importation of slaves from Africa.[59]

The decision to import Negro slaves could not have been made had not conditions within Portugal itself been favorable. First, Portugal at that time possessed important commercial and mili-

[57] Marchant, *Barter to Slavery*, 82-84.

[58] The Portuguese crown issued *cedulas* prohibiting enslavement of Indians in 1570, 1605, 1609, 1648, 1655, and 1686. Nevertheless, Indians continued to be used as slaves throughout the eighteenth century. See, for example, Matias C. Kieman, *The Indian Policy of Portugal in the Amazon Region, 1614-1693* (Washington, D. C., 1954).

[59] Although much has been written about Negro slavery in Brazil, a comprehensive and systematic study of the institution remains to be written. See for widely varying viewpoints, Gilberto Freyre, *The Masters and the Slaves: A Study in the Development of Brazilian Civilization* (2d Eng. ed.; New York, 1956); and Charles R. Boxer, *Race Relations in the Portuguese Colonial Empire, 1415-1825* (Oxford, 1963). See also, Stanley Stein, *Vassouras: A Brazilian Coffee Country, 1850-1900* (Cambridge, 1957); Jose Honorio Rodrigues, *Brazil and Africa* (New York, 1965); and Florestan Fernandes, *The Negro in Brazilian Society* (New York, 1969). Of lesser value are Nina Rodrigues, *Os Africanos no Brasil* (2d ed.; São Paulo, 1935); Donald Pierson, *Negroes in Brazil: A Study of Race Contact at Bahia* (Chicago, 1942); Arturo Ramos, *The Negro in Brazil* (Washington, D. C., 1951); and Mary M. Williams, "The Treatment of Negro Slaves in the Brazilian Empire: A Comparison with the United States," *Journal of Negro History*, VII (1922), 349-64.

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tary advantages on the coast of Africa, and it was thus much safer and cheaper for her to import slaves than it was for other nations.[60] Second, the Portuguese could expect, with some degree of confidence, a sufficiently high rate of return on their investment in Negro slaves to warrant the expense of importing them. Again, Portuguese experience outside Brazil provided the royal government with a way to ensure this high rate of return. The Portuguese, along with the Italians, had been the leading sugar producers of the fifteenth century. Moreover, by the sixteenth century, the Portuguese had surpassed their rivals in the technological knowledge necessary to produce sugar cheaply and efficiently. All they needed to gain the major share of the world's sugar market was to expand their rather small base of operations on the island of Madeira to a larger and more productive territory.[61] Thus, the happy combination of land suitable for the cultivation of a highly profitable export crop, Portugal's superior technology in the growing and manufacturing of that product, and her ability to provide a substitute labor force guaranteed the king of Portugal a prosperous overseas colony.

As long as the Portuguese could maintain a foothold in Africa and as long as they could maintain their lead in the technology of sugar production, the Brazilian economy was able to flourish. In 1549, there were only a few hundred slaves and perhaps only a dozen sugar mills in Brazil. In 1580, there were probably 5000 slaves and 118 *engenhos de açucar* producing about 9,600,000 pounds of sugar each year. By the 1620s, Brazil had roughly 40,000 slaves, with an additional 15,000 being imported each year, and approximately 230 sugar mills producing 22,400,000 pounds per year.[62]

The decision, or rather the nondecision, of the English government as to the direction of the Virginia colony was strikingly different: the English crown simply let the Virginia settlers continue to grope for a way to survive. Left to themselves, Virginians gradually became dependent upon tobacco as a substitute for

[60] Furtado, 10-11.

[61] *Ibid.*, 6-10. For a detailed history of the growth and manufacture of sugar, see Noel Deerr, *The History of Sugar* (London, 1949).

[62] Marchant, *Barter to Slavery*, 123-28; Simonsen, 168-77; Charles R. Boxer, *Salvador de Sá and the Struggle for Brazil and Angola* (London, 1952), 178-80.

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the gold and silver that had eluded them. And they grew that tobacco themselves. Instead of trying to befriend the Indian or even to capture and enslave him, the Virginians declared a policy aimed at his total extermination. Each year after 1624, organized expeditions fanned out from the Jamestown area bent on destroying as many Indian crops and towns as possible. The English settlers of Virginia had decided that if they could not profit from their Indian neighbors, then the only other alternative was "perpetual enmity" with them.[63]

Nor did the Virginians turn, at least in the first century of the colony's existence, to Africa for a labor supply. They did not make any major investments in African slaves because they lacked precisely those advantages that the Brazilians possessed. First, England did not engage in a major way in the international slave trade until 1672 when the Royal African Company was organized.[64] Thus, Virginians in the early seventeenth century found it impossible to import slaves cheaply to the New World. Second, the English crown did not believe that the economy of seventeenth-century Virginia was capable of supporting the investment necessary for the massive importation of Negro slaves. The Spanish were far more advanced than the English in both the cultivation and the marketing of tobacco.[65] The English, because they did not possess the necessary technology and because the royal government initially distrusted the crop, were unwilling to risk a major investment in order to purchase slaves for the Virginia enterprise.[66]

To be sure, African slaves were imported into Virginia from 1619 on, and by the 1660s their presence was obvious enough

[63] Craven, *Southern Colonies*, 172-74.

[64] C. M. Macinnes, *England and Slavery* (Bristol, 1934), 20-29; Eric Williams, *Capitalism and Slavery* (Chapel Hill, 1944), 30-31; Kenneth G. Davies, *The Royal African Company* (London, 1957), 38-46.

[65] Strachey, 122-23; Lewis C. Gray, *History of Agriculture in the Southern United States to 1860* (Washington, D. C., 1933), I, 21-22.

[66] The case of the British in the West Indies is roughly similar. As long as the West Indian colonies' staple crop was tobacco, the number of Negroes imported annually remained small. When in the 1640s, the British West Indies began to produce sugar, which was more profitable than tobacco, the volume of the slave trade increased. The supply of slaves could not equal the demand, however, until the 1670s when the British finally participated in a major way in the international slave trade.

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to warrant the kind of proscriptive legislation that was to eventually strip them of nearly all human rights. But for over a century after the settling of Jamestown, white labor, indeed, free white labor, dominated the Virginia labor force. In 1624, there were only 23 blacks in a population of 1,275. By 1670, the black population had risen to 2,000, but the indentured servant population numbered 6,000, and the free white population was 20,000-25,000. By the turn of the century there were only 6,000 Negroes in a total population of over 70,000. There is no question but that the tide started to turn at that point. By 1742, Negroes constituted one-third of Virginia's population; by the Revolution they accounted for over 40 per cent of the population.[67] Yet the fact remains that during the first one hundred years of the Virginia experiment, the English were unwilling to risk an investment in Negro slaves and the labor force of seventeenth-century Virginia remained predominantly free and white.

Furthermore, the economy of Virginia during the seventeenth century was not a large-scale plantation economy, but one of middle-sized farms with landholdings averaging about 400 acres and only a few plantations over 2,000 acres.[68] Thus, while in Brazil the number of large sugar plantations rose in proportion to the rise in the Negro slave population, in Virginia the average landholding remained small as long as Negro slaves played only a subsidiary role in the economy.

This divergence in the composition of the populations of late sixteenth-century Brazil and early seventeenth-century Virginia signaled much more than a mere difference in the economic bases of the two regions. Economic considerations were paramount in shaping the initial decision regarding the nature of the labor force in each colony, but it was the consequences of that economic decision and not the considerations behind the decision that would have the most lasting effects on the cultures of the two colonies. Indeed, Virginia would eventually resort to a large-scale, single crop, commercial agricultural system much like that of Brazil,

[67] Gray, I, 351-53; Philip A. Bruce, *Economic History of Virginia in the Seventeenth Century* (New York, 1896), I, 572-73; Evarts B. Greene and Virginia D. Harrington, *American Population Before the Federal Census of 1790* (New York, 1932), 137-41, 154-55.

[68] Thomas J. Wertenbaker, *The Planters of Colonial Virginia* (Princeton, 1922), 53-59.

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but by that time the ethnic and cultural differences in the populations of the two areas would be fixed.

Although it is unwise to accept completely Gilberto Freyre's romanticized account of Brazilian slavery and the resultant creation of a new "Luso-tropical" race from the blending of Indians, Negroes, and Europeans, it is nevertheless undeniable that the Portuguese in Brazil, for a variety of reasons, were much less averse to working, worshipping, eating, and cohabiting openly with the Indians and Negroes than were their English counterparts in North America. Both contemporary testimony—plantation records, diaries, and travelers' accounts—and subsequent developments—the influence of the Indian and Negro on eating habits, speech, religion, and, most obvious, on the complexion of the Brazilian population—attest to the evolution of Brazil into a more racially mixed society than any other colony in North America.[69]

The various explanations advanced by historians for this difference have become the subject of heated, and increasingly fruitless, debate. The "conscience" of the Catholic Church, the relative liberality of Portuguese slave codes, the paternalistic, noncapitalistic bent of the Portuguese colonizer, the dearth of Portuguese women in Brazil, and even the virility of the Portuguese

[69] Gilberto Freyre, *Masters*, and Freyre, *The Mansions and The Shanties: The Making of Modern Brazil* (New York, 1963), is responsible for changing not only the trend of historical writing on the Negro, but also, I suspect, is at least partially responsible, because of his "Luso-tropical" nationalism, for softening the attitudes of contemporary white Brazilians toward their Indian and Negro countrymen. Freyre, by boasting about his racially open society and by his considerable prestige within that society, has helped to create a self-fulfilling prophecy. He is supported in his views by, among others: Pierson, *Negroes in Brazil;* Frank Tannenbaum, *Slave and Citizen: The Negro in the Americas* (New York, 1947); Stanley Elkins, *Slavery: A Problem in American Institutional and Intellectual Life* (Chicago, 1959). On the other hand, Boxer, *Race Relations;* David B. Davis, *The Problem of Slavery in Western Culture* (Ithaca, 1966); Marvin Harris, *Patterns of Race in the Americas* (New York, 1964); and, to a lesser extent, Carl Degler, "Slavery in Brazil and the United States: An Essay in Comparative History," *American Historical Review,* LXXV (1970), 1004-28, are among the many historians who argue that slavery in Brazil and Spanish America was much harsher than the former have portrayed it. It is possible, however, to accept Boxer's, Davis's, Harris's, and Degler's less romantic view of the operation of slavery in colonial Brazil and still be persuaded that Negroes, Indians, and whites coexisted more harmoniously in Brazil than in North America.

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male are among the many reasons cited for the differences between the Brazilian and North American racial situations.[70] Some of these "outside" influences from Europe were undoubtedly important in the creation of the distinctly different cultures of Brazil and Virginia, but they obscure the central and obvious fact that the Portuguese settlers of Brazil, because of their initial decision to make the Indian and Negro an indispensable part of their labor force, opened up at the very beginning the possibility of creating a genuinely multiracial society. By opting at an early stage for a system of labor that was potentially degrading and dehumanizing for the Indian and African slave, the Portuguese, paradoxically, made it impossible in their society to avoid constant contact with the Indian and the Negro and therefore impossible to avoid familiarity among races and, ultimately, impossible to avoid at least some relaxation in the white man's rigid and discriminatory social system.

Virginia, by pursuing an official policy of extermination of the Indians and an unofficial policy of nonimportation of Negro slaves, kept itself "English" in its cultural attitudes and habits. Seventeenth-century Virginians because they were not suffered to come in contact with either the Indian or the African were allowed to retain their old, Elizabethan prejudices concerning other cultures.[71] By the time Virginians had amassed enough capital and had assured themselves that the slave system would be profitable, their racial attitudes had been set. After 1700, their society would no longer be free of "foreign" cultural influences, and it would be difficult, even for the nonslaveholding yeoman farmer, to avoid

[70] See for example, Tannenbaum, 48-62, 82-90; Elkins, 52-80; and Freyre, *Masters*, 29, 185, 278, 330, 370-72, 441-52. Freyre goes so far as to say that the original settlers of Brazil were unusually oversexed:

> To the wilderness, so underpopulated, with a bare sprinkling of whites, came these oversexed ones, there to give extraordinary rein to their passions. . . . Attracted by the possibilities of a free and untrammeled life, with a host of nude women around them . . . many Europeans proceeded to settle here out of predilection or of their own free will. Unbridled stallions is what they were.

[71] Winthrop Jordan, *White Over Black: The Development of American Attitudes Toward the Negro, 1550-1812* (Chapel Hill, 1968) is the one indispensable source explaining the complex process by which Englishmen in America came to reinforce and intensify their preexistent prejudices toward the Negro.

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contact with the Negro. But by 1700, the settlers of Virginia were already committed to a society where the white European would be the dominant cultural element. The white man in Virginia, faced with the practical necessity of living in a multiracial society, would respond either by ignoring the existence of other races or, when they could not be ignored, by erecting institutional barriers between the races.

It was at this point, then, that the Brazilian and Virginian societies began to diverge most dramatically. Both the Portuguese and English desired quick and easy riches in the New World. Both found it difficult to fulfill that desire. But Brazil, because of Portugal's position in the international slave trade and her superior technology in the production of sugar, was able to keep alive the dream of a colony based on the work of others. It is one of the paradoxes of the history of colonization in the New World that the Portuguese in Brazil, unlike the English in Virginia, began the slow (and still uncompleted) task of developing relatively humane and equitable codes of behavior toward other cultures while in the very process of exploiting the people of those cultures.

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Giolitti's Reform Program: An Exercise in Equilibrium Politics*

SÁNDOR AGÓCS
Wayne State University

On March 10, 1913, Giovanni Giolitti startled Italy by resigning as prime minister. Giolitti's retreat from the summit of political leadership may appear to be one of the paradoxes of modern Italian history, for his abrupt withdrawal closely followed one of his greatest triumphs, the passage of the Electoral Reform Act of 1912, which he had ushered through parliament against considerable opposition. But the impression is deceiving. A close look at the circumstances of Giolitti's fall reveals the seeming paradox to be a fine historical irony, since Giolitti's very success in seeing through parliament a reform program—of which the Electoral Reform Act was the culmination—was the immediate cause of his downfall.

Giolitti's ability to muster the votes in the Chamber for the reforms he deemed necessary established him as the undisputed political leader of Italy for over a decade. His program of reforms also made him the most significant Italian practitioner of European New Liberalism. Giolitti did not contribute theoretical works to this new intellectual current, but he put into practice several of the tenets of New Liberalism before some of the theorists of the intellectual current had shown awareness of them. And nothing speaks

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better for the timeliness of the changes proposed by New Liberalism than the fact that Giolitti defended his reforms by referring always to the needs and exigencies of Italian life.

A somewhat oversimplified statement might characterize New Liberalism as the revitalization of the liberal doctrine by the infusion of a social conscience that was conspicuously lacking in old liberalism.[1] In Giolitti's practice this was manifested in speeches and votes in the Chamber in favor of legislation that aimed at improving the lives and working conditions of the laboring classes by shortening working hours, preventing child labor, guaranteeing sickness and retirement insurance, and reducing the tax burden of workingmen.[2]

The Electoral Reform Act, while it was the culmination of Giolitti's legislative program, also represented a dramatic change in the pattern of Giolittian reformism. Unlike the other bills Giolitti sponsored, it was not a piecemeal improvement in the economic conditions of the working classes. The act introduced nearly universal manhood suffrage. It brought the Italian masses onto the political scene. But Giolitti's success in seeing the Electoral Reform Act through parliament became his undoing, for his method of political operation was not geared to the new political situation created by the extension of the suffrage. His political practice, characterized by electoral manipulations and backstage deals, was the politics of a select few. His was the nineteenth-century style of government by *notables* that became obsolete in the twentieth-century world of mass politics and mass parties.

The "old fox" of Italian political life, who outmaneuvered all the other politicians for over a decade, apparently outmaneuvered himself with the new electoral law; and the dramatic spectacle of the old politician left behind by events he himself set in motion can be explained only through a study of the changes in the social philosophy that motivated his political practice.

[1] Probably the most articulate theoretical statement of European New Liberalism is L. T. Hobhouse, *Liberalism* (New York, 1964), first published in 1911.

[2] For Giolitti's arguments in favor of tax reforms, see his *Discorsi parlamentari, pubblicati per deliberazione della Camera dei Deputati* (Rome, 1953-56), I, 276, 302-03, 516; II, 547 ff., 597, 599-600, 613. Speeches in favor of other social legislation proposed by Giolitti in *ibid.*, I, 393, 498 ff.; II, 669-71, 758 ff., 843-44, 854 ff., 1170 ff. A list of the legislation proposed by Giolitti in the Chamber is in *ibid.*, IV, 1908-2293.

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Giolitti's social ideas underwent important modifications during his long political career that had begun in the 1880s. His view of the working classes, for instance, changed significantly, reflecting a changing understanding of the nature of social classes. In his earliest speeches he tended to characterize classes as professional-occupational groups. Thus in 1882 he called members of the medical profession and municipal administrative employees "classes." He also believed there was a "solidarity of interests" among "classes" of similar occupation. Thus in 1885, Giolitti spoke of the mutual interests that tied landowners to agricultural laborers.[3] But eventually he abandoned this wishful thinking about the solidarity between capitalists and proletarians.

During the 1890s, as Italian Marxists began to raise audible voices, Giolitti seems to have accepted economic interests related to the ownership of the means of production, rather than profession or occupation, as the defining characteristic of classes.[4] Gradually he came to use terminology somewhat akin to that of the Marxists when speaking of classes. Significantly, until the year 1900 he appears to have preferred terms like "most numerous classes," "the lowest social classes," or the "poor" in connection with the working classes, betraying an affinity in his thinking with the outlook of social conservatives.[5] But after 1900, Giolitti seems to have preferred the term "working classes" for the industrial and agricultural proletariat.[6]

[3] Giovanni Giolitti, *Discorsi extraparlamentari* (Turin, 1952), 91-94; also *Discorsi parlamentari*, I, 2-3.

[4] According to one of the most knowledgeable interpreters of Giolitti, he did study the Marxian doctrine; see Gaetano Natale, *Giolitti e gli Italiani* (Milan, 1949), 73. His private library contained not only the basic writings of Marx, including *Das Kapital*, but the works of many interpreters of Marx, among them Arturo Labriola and Eduard Bernstein; Nino Valeri, "Introduction" to Giolitti's *Discorsi extraparlamentari*, 78-79. In stressing the need for an equitable system of taxation, on several occasions, Giolitti described turn-of-the-century Italy in terms similar to those of contemporary Marxists: as a paradise for the rich who were taxed to the minimum while they exploited to the utmost the industrial and agricultural proletariat. Even the law served the rich alone, Giolitti pointed out, for justice was so expensive that it remained outside the reach of the lower classes; *Discorsi extraparlamentari*, 206 ff. and *Discorsi parlamentari*, II, 676 ff.

[5] For examples, see *Discorsi extraparlamentari*, 92; *Discorsi parlamentari*, I, 302, 504; II, 547-52.

[6] For examples, see *Discorsi parlamentari*, II, 632, 660 ff., 676, 686 ff., 1200-06; III, 1536 ff. In Giolitti's *Discorsi parlamentari*, I encountered refer-

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Even during the early phase of his political career, the terms Giolitti used to refer to the working classes implied the existence of the proletariat as a distinct group, but he was slow in recognizing it as a fully autonomous force. His earliest outlook was not entirely free from the paternalism of contemporary conservatives who placed the proletariat under the "tutelage" of the "superior" classes.[7] But Giolitti freed himself from the confines of this position early in his political career. As early as the 1890s he recognized the constitutional right of Italian workers to build their own organizations.

As they gained strength, Italian labor unions came to be, as Giolitti hoped they would,[8] essential in the Italian working classes' struggle to gain an equitable share of the wealth in society. Unlike conservatives, who showed no concern about the social distribution of wealth and presumed that the contrast between haves and have-nots was providentially arranged, Giolitti saw a danger in the accumulation of wealth in a few hands. And he came to regard labor struggle and strikes as the means to bring about a balanced distribution of the fruits of the economic progress of the country. Thus amid protest from conservatives, he calmly and consistently rejected suggestions that he suppress the labor unions and strikes.

ences to the "working classes" 12 times prior to 1901 and 23 times after 1901. The distribution of this partial account of the specific terms used by Giolitti in reference to the concept of working classes is shown in the following table:

References to the "Working Classes" in Giolitti's Speeches

Terms	*1882-1900*		*1901-1914*	
Old style or conservative	7	59.0%	7	30.5%
"poor," "poorer"	3		1	
"less wealthy"	1		5	
"low," "lowest"	3		1	
Neutral	2	16.0%	7	30.5%
"popular"	1		6	
"most numerous"	1		1	
New style, Marxist-oriented	3	25.0%	9	39.0%
"working classes"	3		6	
"proletariat"	0		3	
Totals	12	100%	23	100%

[7] See the "Manifesto dell'opposizione subalpina," dated April 28, 1886, in *Discorsi extraparlamentari*, 95 ff.; also Valeri, "Introduction," *ibid.*

[8] *Discorsi parlamentari*, II, 626 ff., 636 ff.; also Natale, 109 ff.

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This was Giolitti's position as early as 1892-93, during his first premiership. In those years, labor unions—*fasci*—were organized in Sicily by miners and agricultural laborers, and strikes broke out in numbers unknown in the history of the island. A careful study of the strikes left Giolitti convinced that the extreme misery of the Sicilian working classes was the most important cause of the strikes. The disparity between the living standards of the upper classes and the workers was more extreme in Sicily than in any other part of Italy.[9]

Giolitti saw the social movement embodied in the Sicilian *fasci*, as he was later to regard the upsurge of the labor movement on the mainland, as an economically motivated action of the proletariat aimed at improving their material conditions.[10] Thus instead of ordering the police to break up the labor organizations by force, he instructed local authorities to restrict police action to the maintenance of public order.[11] He insisted on this policy even when it became clear that his government might consequently be forced out by a mounting conservative hysteria in the Chamber.

Giolitti may have been justified in arguing that his conduct during the Sicilian events remained within the bounds of liberal political doctrine, which prescribed that public authorities remain aloof from the struggles of economic competition. But his view that the conflict between Sicilian workers and their employers was a specific instance of the free competition of liberal economic life was inconceivable within the confines of old-style liberalism. In fact Giolitti incorporated a new element, labor conflict, into the framework of liberal economic theory, considering it a fundamental element of free competition.

Giolitti's acceptance of labor conflict as a fact of life was a consequence of his conviction about the class-structured nature of society. The labor unions represented the economic interests of the working classes,[12] and in a system of free competition, the

[9] *Discorsi parlamentari*, I, 368, 519; II, 1032; III, 1370.

[10] See Giolitti's communication to the police chief of Novara, April 12, 1901, in *Dalle carte di Giovanni Giolitti: quarant'anni di politica italiana* (Milan, 1962), II, 24. *Discorsi parlamentari*, II, 636 ff., 660 ff., 676 ff., 686 ff.; III, 1260 ff.

[11] *Discorsi parlamentari*, I, 368 ff.

[12] *Discorsi parlamentari*, I, 255 ff.; II, 627.

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unions' conflicts with the owners of agricultural and industrial enterprises seemed as unavoidable to Giolitti as conflicts between business competitors.

When his conservative opponents in parliament implied that he was fomenting labor struggle, Giolitti peppered his reply with sarcasm. Accusing him of "encouraging" labor struggle, he argued, was like blaming an astronomer for having "encouraged the moon to pass before the sun" because he foresaw an eclipse.[13]

"I imagine it is hard to walk the world backwards," Giolitti once said, appraising the efforts of a conservative opponent, a Senator Ginistrelli, whose name would probably be unknown today if he had not secured himself a place in Italian history by provoking Giolitti's sarcasm. The senator, who raised horses when he was not busy with the solution of the social question, proposed to outlaw strikes. He should have known better, Giolitti told him; did he not know that it was just about impossible to "walk a horse backwards" and still more difficult to "walk the world backwards"?[14] And this was not Giolitti's last sneer at those who dared to attack his conviction that since the primary aims of the labor movement were economic,[15] strikes were legitimate under Italian law.

Giolitti seems to have believed that economically motivated class conflicts turned into political action only if they were unnecessarily prolonged by the irrational refusal of those involved to accept a reasonable settlement, a fair compromise. In such a case, extremist political parties were bound to move onto the scene and exploit the conflict, turning it into a political one.[16] The mediation of public authorities that Giolitti usually insisted upon was aimed at preventing outbreaks of violence and bloodshed by helping to resolve the conflict at its economic stage.

[13] *Discorsi parlamentari*, II, 686 ff., 696 ff.

[14] For Giolitti's clash with conservatives in the Senate, see his *Discorsi parlamentari*, II, 753 ff.

[15] Sometime later, in the Chamber he questioned the mental equilibrium of those who saw a danger of revolution in an agricultural strike in the Parma region; *Discorsi parlamentari*, III, 1260-61. Giolitti evidently considered general strikes "political," and hence his consistent opposition to attempts at general strikes; see Giampiero Carocci, *Giolitti e l'età giolittiana* (Turin, 1961), 67.

[16] *Discorsi parlamentari*, II, 593 ff., 636 ff., 1169.

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Giolitti argued that he maintained a strict neutrality of the state in labor conflicts,[17] thus intending to show that he adhered to the liberal doctrine which prescribed that public authorities stay out of economic competition. But this must be seen, at least in part, as lip-service paid to principles he tended to disregard in practice. The mediation of public authorities more often than not favored the workers and implied pressure on the employers to accede to their demands. This was inevitably so when Giolitti let his subordinates know that he considered the workers' demands justified.

The key to this apparent contradiction between confessed principles and practice might be found in a speech Giolitti made in the Chamber on March 14, 1902, at a time when labor conflict was reaching proportions unseen before in Italy. The government, that ideally should stay out of labor conflicts, had to intervene as a "peacemaker," he argued, because proper legislation regulating the conflicts, and presumably protecting the rights, of the workers was not yet enacted. Until commissions were created to ensure justice in labor disputes and their activities circumscribed by laws, he explained, public authorities would have to assume the role of mediator.[18] Giolitti made it clear, as he did on other occasions, that he considered this "social apostolate" of state functionaries a bold experiment, a hard road to travel, but necessary for the sake of "social peace."

As a consequence of his commitment, Italy saw his subordinates, the local police chiefs among them, engaged in the most unexpected of activities—social research. And some people who called themselves sociologists could have learned a thing or two from the seriousness with which those policemen tried to untangle the implications of labor conflicts and from the depths of their investigation of the living conditions of the working classes.

On April 24, 1901, for instance, the prefect of Mantova, Adami Rossi, sent to Giolitti a six-page report concerning a local strike.[19] While this document might have been unusual for its

[17] *Discorsi parlamentari*, II, 626 ff., 662 ff., 704 ff.

[18] *Discorsi parlamentari*, II, 688 ff. For examples of Giolitti's continued concern with this problem, see *ibid.*, 936, 1005, 1072 ff.; *Dalle carte di Giovanni Giolitti*, II, 38-40, 296.

[19] *Dalle carte di Giovanni Giolitti*, II, 30-36. Giolitti used the data provided

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length, it was a routine part of the work Giolitti expected from police chiefs. The prefect must have had the information on hand because Giolitti's request for the data came on the previous day, and no policeman-turned-sociologist could gather in twenty-four hours the complex information that the report manipulates with ease. Adami Rossi obviously not only had the data at his fingertips, he had been doing some careful thinking about them. For his report sets out to explain the "genesis" of the labor organizations that were struggling to better the conditions of agricultural laborers in the Mantova region. The prefect saw the origins of the labor struggle in the fact that the incomes of landowners had been steadily increasing while the same was not true for the hired hands who worked the lands. As a matter of fact, Adami Rossi showed that they did not earn enough for their families' basic necessities. His scrupulously assembled "microdata," details concerning average daily wages and average cost of housing and of food requirements, seem to bear out his conclusions convincingly. Adami Rossi's report shows, moreover, that he was not one of those types who wrote local history as a hobby. For he concluded that, since the workers employed in similar categories in other parts of Italy earned more than those of the Mantova region—and again his arguments were supported by carefully tabulated statistical data—he found their demands for an increase in wages justified.

Giolitti's assignment to local authorities of the role of arbitrator in social disputes, their "social apostolate," was meant to ensure that the changes he deemed necessary were carried out on all levels. He was thus attempting to keep a newly emerging sociopolitical force, the proletariat, within the bounds of legality by ensuring that they were granted whatever was "justified" in their demands. He intended to prevent the Italian working classes, and with them the whole of society, from becoming "victimized" by revolutionary propaganda.[20] But he was hoping to achieve more.

by Adami Rossi in a speech to the Senate four days after the prefect's report was forwarded. Although the Mantovan "sociologist" did not get a footnote for his research and his name was not even mentioned, Giolitti testified to his research methodology. He said the *prefettura* had to collect the data from landowners and mayors, because no centrally collected statistical information was available; *Discorsi parlamentari*, II, 636-43.

[20] *Discorsi parlamentari*, I, 507 ff.; II, 636 ff., 1161 ff.

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He expected to be able to dispel the growing conviction among the workers that the government was serving only the interests of the bourgeoisie, that it was a government of the upper classes.[21]

In spite of these loudly proclaimed aims, Giolitti's economic and social reform program clearly served to perpetuate the oligarchial political rule of the bourgeoisie, for his reforms implied that workingmen, who were almost entirely disfranchised then, would take up political activities only as a last resort, if they could not otherwise assure their birthright, economic progress.[22] This was the sustaining idea behind Giolitti's arguments when he attempted to sell his economic and social reforms to the reluctant representatives of the bourgeoisie in parliament. He argued that without the reforms a revolution would be forthcoming.[23] This argument in turn assumed that the upper classes would retain political control as long as they were willing to accept the economic sacrifices involved in selected reforms aimed at improving the material conditions of the working classes.

Giolitti's conception of political class is suggested by the fact that on several occasions he used the words "ruling classes" and "richest classes" interchangeably, and the term "bourgeoisie" in connection with the "ruling classes."[24] Especially during the early phase of his long political career, he clearly viewed politics as the activity of an oligarchy, of a small group drawn from the rich and versed in the *sprezzatura* of political fencing and rhetoric. But even in his memoirs, written late in his life, Giolitti's descriptions of political figures emphasized the ability to "dominate" assemblies as the mark of a successful politician. Characteristically, he also pointed to his own good luck in having had excellent fencing masters in his youth, adding that this served him well later in "parliamentary fencing."[25]

Giolitti shared with his conservative contemporaries the idea of politics as the activity of a select few. Whether they felt this small echelon should be drawn from the rich or from a wider segment of the population was expressed by the deputies in their

[21] *Discorsi parlamentari*, II, 593 ff., 661 ff., 679, 686.

[22] *Discorsi parlamentari*, II, 547 ff., 594, 660, 676 ff., 687, 703-05, 1169-70, 1186.

[23] *Discorsi parlamentari*, II, 547-48, 594, 632-33, 695.

[24] *Discorsi parlamentari*, II, 676, 1202.

[25] Giovanni Giolitti, *Memorie della mia vita* (Milan, 1922), 8, 57.

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choice of seats on the right or left side of the Chamber. And legislative, parliamentary activities, not political parties, provided the framework of Italian politics in the era of Giolitti. Soon after delivering their maiden speeches in the Chamber, the deputies found themselves in the grip of Giolitti's *trasformismo*—backstage deals buying off the votes of representatives with favors promised by the government.

It can be argued that Giolitti's method of political operation has always characterized parliaments; pork-barrel deals were not unique to the Italian Chamber. The system of *trasformismo* was not "established" by Giolitti; he merely followed time-honored parliamentary practices. Bribing the deputies with legislative favors, he continued the nineteenth-century system of politics as the activity of notables, politics based on personalities, rather than on political parties, which would come to dominate twentieth-century European parliaments.[26]

Giolitti's political practice clearly shows that he lacked a modern concept of political party. With its nineteenth-century method, Giolittian *trasformismo* might be taken as a sign of "backwardness" on his part, for our concept of political party was shaped by writings that appeared while Giolitti's political career was at its apogee. Furthermore, some of the activities of Robert Michels, one of the leading theoreticians of modern political parties, took place in Italy under Giolitti's eyes.[27] Giolitti's political practice suggests either that he did not read Michels or that he found his arguments unconvincing. Unlike Michels, Giolitti never came to conceive of political life as evolving within the framework of political parties with strictly drawn ideological borderlines.

Giolitti consistently identified his own ideological outlook as "liberal," but his definition of liberal ideology remained rather vague, a cover for whatever political program he deemed neces-

[26] A contemporary spoke of "occult maneuvers" on Giolitti's part; see Filippo Crispolti, *Politici, guerrieri, poeti: ricordi personali* (Milan, 1938), 69. On Giolitti's parliamentary method of operation, see A. William Salomone, *Italy in the Giolittian Era: Italian Democracy in the Making, 1900-1914* (Philadelphia, 1960), 15, 30, 108 ff.; Luigi Salvatorelli, "Giolitti," *Rivista Storica Italiana*, LXII (1950), 525-27.

[27] Robert Michels, *Political Parties: A Sociological Study of the Oligarchical Tendencies of Modern Democracy* (New York, 1968); this was first published in several languages, Italian among them, during the years before World War I.

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sary for Italy at any given time. In the name of liberalism he urged reforms aimed at improving the material conditions of the working classes, reforms that could be seen as attempts to perpetuate oligarchy by defusing workers' discontent. But the word *liberal* was also on his lips when, during the second phase of his political career, he pressed for the democratization of Italian political life and demanded an authentic voice for the proletariat in the political affairs of the country.[28]

Although Giolitti did not consider liberalism a compact and well defined system of ideas like the modern ideologies Michels described, liberalism performed for Giolitti a function similar to the one Michels assigned to modern ideologies. It held Giolitti's Liberal party together. Yet the *partito liberale* was not a political party in the modern sense of the word. It was little more than a group of deputies in the Chamber who, like Giolitti, professed to adhere to liberal ideals. The immediate aims of parliamentary tactics and particular legislative maneuvers were almost as important in holding this political group together as a shared ideology.

Giolitti's role in perpetuating this state of affairs became an issue raised by many of his critics. Luigi Sturzo, the future leader of Italian Christian Democracy, declared the lack of political parties "one of the fundamental defects of our parliament." And Romolo Murri, another young Christian Democrat, like Sturzo, emphasized the need for clear ideological divisions in political life and considered the lack of disciplined and ideologically motivated political parties the curse of Italian politics during Giolitti's era.[29] Gaetano Salvemini, a socialist critic of the Giolittian system, also decried the absence of political parties on the Italian political scene, arguing that the social and political problems of the country could not be resolved without the "pressure of the interested classes" exercised through political parties, the "springs of political action."[30]

[28] For a variety of the programs Giolitti labeled "liberal," see his *Discorsi parlamentari*, I, 511; II, 696 ff., 1188 ff., 1471 ff.

[29] Sturzo is cited in Franco Rizzo, *Luigi Sturzo e la questione meridionale nella crisi del primo dopoguerra* (Rome, 1957), 10; Murri is quoted in Giampiero Cappelli, *Romolo Murri, contributo per una biografia* (Rome, 1965), 93-94.

[30] Salvemini is cited in Rosario Villari, "Il meridionalista," in R. Villari *et al.*, *Gaetano Salvemini* (Bari, 1959), 141-42.

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Ironically, Giolitti, when he was a young politician reaching out for power, likewise deplored the "lack of rational and logical organization of political parties."[31] But after he gained power, Giolitti seems to have forgotten his earlier arguments. He did not share Salvemini's concern about the need for mass parties because Giolitti's political method was geared to a political system based on a very limited suffrage that excluded the masses from political life.

Giolitti continued to use the *trasformismo*. It was not a change in political practice, but a more viable and up-to-date program that allowed him to dislodge his predecessors and become the political leader of Italy. Giolitti set out to put Italy on a political course that was not foreseen by old-style liberalism. The criticism that he was modifying liberalism beyond recognition was more than the outcry of political purists, for his program, especially after 1900, carried innovations that were inconceivable within the confines of the old liberalism.

Giolitti's undogmatic attitude, which held liberalism to be a principle of life rather than an abstract and sacrosanct theory, was behind his effort to press outward the boundaries of liberal ideology.[32] He saw in liberalism a flexible complex of principles that could be modified to suit changing conditions in society, or even abandoned and replaced by new ones. Thus in 1901, Giolitti pointed to the need to extend the narrow political base of the Italian state, arguing that advancement toward an equalitarian political system was an organic part of the progress of modern society.[33] But the concession of full political rights to all men was not an easy step for Giolitti to take. His reluctance was not only due to his political realism, to his knowledge that a proposal for universal suffrage would encounter stiff resistance in the Italian parliament. It also stemmed from his own reservations, for the political traditions of old liberalism weighed heavily upon his outlook.

In emphasizing gradualism in the economic and political ad-

[31] See his electoral speech on October 29, 1899, in *Discorsi parlamentari*, II, 1194 ff.

[32] Natale, 104.

[33] *Discorsi parlamentari*, II, 633; III, 1536 ff. Gabriele De Rosa, "La crisi del sistema giolittiano," *Rassegna di politica e storia*, I (1955), No. 8, 4-11 and No. 10, 6-11; Salomone, 107, 135 ff.

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vancement of the working classes, Giolitti testified to his acceptance of one of the basic tenets of traditional liberal doctrine. Furthermore, not unlike mid-nineteenth-century liberals, he seems to have assumed that property would somehow guarantee a responsible political attitude.[34] The position he took on the issue of education and "intellectual maturity," the other preconditions of a politically responsible attitude, also shows Giolitti sharing the view of liberal traditionalists. In his parliamentary speeches he often referred to the importance of education in terms akin to those of traditional liberals. He tended to take violence or even political unrest for a sign of political immaturity. Although the working classes' desperate material conditions, and especially their inability to improve their lot, had an important bearing on Giolitti's understanding of the workers' occasional resorts to violence, he tended to see in such actions signs of the workers' political immaturity, which he tried to explain, in turn, by pointing to their lack of education.[35]

Many years were to pass before he argued, as he did in 1912 when he proposed the introduction of nearly universal manhood suffrage, that more than formal schooling and literacy were needed to assure the development of political responsibility among the masses and that education by a "regime of liberty," that is "the experience of political action within the framework of legally guaranteed rights," was a precondition for the growth of a politically responsible attitude among the workers.[36]

Giolitti's remarks in his memoirs about the extension of suffrage tell of his belief in the intellectual maturity of the Italian people, in their ability to make the right choice when choices were given. This *post factum* justification of his political action bespeaks his belief that reason and good sense would "prevail in the end" among the people.[37]

His belief in the people's natural intelligence, rationality, and down-to-earth solidity, Giolitti's populism, was to become the ultimate justification of his drive for the democratization of the country; it sustained his arguments for the viability of an equali-

[34] *Discorsi parlamentari*, III, 1234, 1373, 1466, 1670.
[35] *Discorsi parlamentari*, II, 658, 741-42, 754, 761, 838-39, 862-63; III, 1233.
[36] *Discorsi parlamentari*, III, 1536 ff.; *Memorie*, 176, 311 ff.
[37] *Memorie*, 308.

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tarian political system for Italy. In fighting conservative resistance to his reforms, he would point to the political maturity of the masses. He found occasion to remind even such champions of the people as the socialist leaders when their nagging doubts about the political maturity of the masses drove them into the opinion that the "Italian people were still, in many respects, children." Filippo Turati, the author of this patronizing insinuation did not get away without a lecture from Giolitti, who told him that there was more common sense among the masses than was "generally believed."[38]

Populist convictions such as these sustained Giolitti's drive to extend political rights to all men, but his *trasformismo* gave them the lie. For he did his best to manipulate the electorate just as he did the deputies in the Chamber. The portfolio of the Ministry of the Interior—and the control over the police—which he usually secured for himself when he was premier, provided Giolitti with an opportunity for electoral manipulations. The police chiefs who collected social statistics for him had more than once become unofficial campaign managers for Giolitti's candidates. And after elections, Italy resounded with complaints about police interference and violence committed against the opponents of Giolitti's candidates and their supporters while the police closed their eyes. In a passionate exposé, Salvemini called Giolitti the "Minister of the Underworld" and cited cases of connections between *mafiosi* and some of Giolitti's favored candidates.[39] The fact that these charges were not unfounded served to neutralize the high-sounding principles of Giolittian populism. At least in some districts, elections became a mockery of the idea of free electoral choice, and Giolitti must have been aware of this.

The notion of politics that confined the fate of the body politic to an upper-class elite willing to accept the sacrifices involved in a program of economic and social reforms was slow to give way in Giolitti's thinking to the widening of the Italian political base by the extension of suffrage to the working classes. Giolitti's two platforms of social and political reformism telescoped into each

[38] *Memorie*, 176, 192 ff.

[39] Gaetano Salvemini, "Il ministro della mala vita," first published in 1910, reprinted in *Opera Omnia*, ser. 4. (Milan, 1962), I, 73-141; also Salomone, 108 ff.

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other. There is no clear dividing line, but the year 1900 is probably more than an arbitrary marker in the evolution of Giolitti's political outlook and practice. From then on, he came to a step-by-step acceptance of the notion of extending full political rights to Italian workmen.[40]

Giolitti's initial reluctance is to be explained by his earlier adherence to the old liberal political doctrine that allowed the extension of political rights only to property holders. But he seems to have slowly recognized that the Italian working classes could not possibly wait for their right to vote until they became *beati possedenti*. He must have also realized that the industrial proletariat, working for subsistence wages or less, could hardly hope to ever meet the property qualifications of old-style liberalism, especially not in view of the stubborn resistance of the ruling classes to Giolitti's program of economic reforms.

His exasperation with this resistance was probably the most decisive element in Giolitti's turn to political reforms. By including the working classes in the electorate, he obviously hoped to weaken the political position of the bourgeoisie, thus enabling him to clear through the legislature the social reforms he considered indispensable for keeping Italy abreast of the social and economic development of the most advanced European countries.[41]

Giolitti's notion of Italy's progress seems to have rested throughout his entire political career on an equilibrium model. In his dialectic of politics, the conservative Right which resisted changes and the Left which pressed for them played equally important roles. The Left injected the element of forward thrust into political life, while the Right provided the inertia, the resistance to changes that would cause tears in the fabric of a society unprepared for them.[42]

In the spectrum of socio-political forces the Center, where Giolitti positioned himself,[43] played a pivotal role; it balanced the forces of forward-moving thrust and inertia by absorbing ele-

[40] For a review of Giolitti's changing attitude toward universal suffrage, see his *Discorsi parlamentari*, II, 838; III, 1232 ff., 1364 ff., 1707-08. For his final defense of the Electoral Reform Act of 1912, see *ibid.*, 1463-1513, 1536 ff.

[41] *Discorsi parlamentari*, III, 1536 ff.

[42] *Discorsi parlamentari*, I, 498 ff.; *Memorie*, 214, 277-78, 293.

[43] *Discorsi parlamentari*, II, 660 ff.

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ments of both. If one of the two were to gain a domineering position in political life, the moderate Center should ally itself with the other in order to assure a proper momentum in political development. Giolitti's repeated attempts to convince the socialists to accept positions in his government suggest that he considered it necessary to support the forward thrust in social and political developments during the first years of the twentieth century.[44]

Giolitti's idea of incorporating into his platform "whatever was reasonable in the socialist program" might have led conservatives to accuse him of fomenting revolution, but he considered himself consistently liberal throughout his life. While he did not hesitate to "seize" elements of other ideologies when political exigencies made such a move convenient, he never abandoned his adherence to liberalism. He saw in liberalism the principles that should govern the social and political life of the country and considered other ideologies to be complements of liberalism and subject to liberal exploitation when the situation so warranted. Other political forces were but auxiliaries to liberalism in propelling society forward in its progress.

The "force of facts" that his pragmatism led Giolitti to respect drove him toward political cooperation with the Catholics. After the electoral reform of 1912, the liberal-Catholic alliance became a necessity for Giolitti if he wanted his parliamentary Center to remain in control of the Italian political situation. The Catholics, who usually supported conservative positions, were to help preserve a balance in political life by countering the gains socialists were bound to make after most Italian workers received the right to vote. Thus the Gentiloni Pact of 1913, an electoral alliance between Catholics and Giolittian liberals, became an element in Giolitti's scheme of equilibrium politics. The pact, which accomplished the political miracle of electoral cooperation between traditional opponents, marked the apogee of Giolitti's political career. Yet, ironically, immediately after the 1913 elections, Giolitti's secure grip over Italian politics was lost.

His *trasformismo* with its implied manipulation of votes was geared to a system of limited suffrage. The millions of new voters proved to be impossible to manipulate, and the number of radical deputies who refused to play the game of *trasformismo* in the

[44] *Discorsi parlamentari*, II, 593 ff.; *Memorie*, 31 ff., 61 ff.

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Chamber increased. After the 1913 elections, the first to be held under the new electoral law, the Chamber reflected the real mood of the country—dissatisfaction with Giolitti's *trasformismo*. The Italian parliament resounded with protest against Giolittian methods, and the source of the greatest indignation was the shrewdest of Giolitti's backstage deals, the Gentiloni Pact.[45]

Sensing that he would be voted down in the Chamber, Giolitti resigned.[46] The master politician of Italy withdrew at the moment of his greatest triumph, after breaching the divide that had separated Catholics and liberals for decades. He was forced to retreat after the boldest expansion of his program, the extension of suffrage to millions of Italian workingmen.

Giolitti lost because he regarded politics as a game to be played by a select few through backstage deals and considered the masses, at best, manipulated extras on the political scene. He apparently never fully comprehended the twentieth-century world of mass politics and mass parties. The consolation he could find, should he have wanted it, was that he called the masses onto the political scene. Or did he see it as an irony, rather than a consolation, that the storm covering his fading footprints in history was of his own making?

[45] The pact was shrouded in secrecy. Giolitti never admitted that he had anything to do with it nor that it involved some of his men in the Chamber. But V. O. Gentiloni, the president of the Catholic Electoral Union, claimed that hundreds of candidates signed individual agreements promising to uphold Catholic interests in the Chamber. And it was public knowledge that there were Giolittian liberals among the two hundred deputies, who, Gentiloni asserted, were elected with Catholic votes in the 1913 elections. For Gentiloni's statement, see the interview in *Giornale d'Italia*, 13, No. 310, Nov. 8, 1913. For the recriminations against Giolitti and his reaction, see *Atti del Parlamento Italiano, Camera dei Deputati, Sessione 1913-1914, Discussioni* (Rome, 1914), I, 94-478.

[46] Giolitti probably expected a comeback after tempers cooled in the Chamber. But his last chance at the premiership did not appear until six years later and after yet another obvious failure of his leadership in connection with World War I. Giolitti committed his prestige to an attempt to prevent Italy's entry into the war and failed in this endeavor. His shortlived last premiership in 1920 came following a last disastrous experiment in *trasformismo*, an electoral alliance with Mussolini's Fascist blackshirts. Apparently after the war they were the only ones left in Italy willing to be "transformed" and play the game of Giolittian politics.

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Book Reviews

Economic Growth of Nations. Total Output and Production Structure *by Simon Kuznets. Cambridge, Mass. The Belknap Press of Harvard University Press, 1971. — vi, 363 pp. $15.00.*

Between October 1965 and January 1967, Professor Kuznets published ten massive papers in *Economic Development and Cultural Change* or in supplements to that journal. They summarized his own work and that of others (largely inspired by his example and method) on economic growth and its statistical patterns, covering many countries. His objective was to derive from these materials such generalizations as international comparisons would permit and to identify and measure deviations in patterns of growth.

This book represents his revision, updating, and elaboration of the first three papers, relating to aggregate growth and to shares of production sectors in both product and in the distribution of the working force. (Kuznets's *Modern Economic Growth* [New Haven, 1966] summarizes his interim findings over a wider range.)

International comparisons are of two types: historical and cross-sectional. The United States, Canada, Western Europe, Australia, and Japan provide the bulk of the historical data. Cross-sectional analyses, relating sectoral shares to gross domestic product and the distribution of the working force, are derived from United Nations and other data, mainly of the 1950s. They cover between thirty and fifty countries at different levels of gross domestic product per capita, depending on the availability of data for particular calculations. The method is designed to isolate similarities and differences in growth patterns of the past and of the post-1945 period.

His principal findings are summarized in 22 paragraphs (pp. 303-14).

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Among them are these:

• Total product growth accelerated as nations entered "modern economic growth," starting with great Britain in the late eighteenth century: the last one and one-half or two centuries represent a new economic epoch as compared to the longer past.

• With the important exception of Japan, currently, the developed countries are all in Europe or are European offshoots overseas.

• The currently less developed countries have failed to enter the developed group either because of the low initial levels of their per capita product (as they entered "modern growth") or because of low rates of growth in per capita product during the past century or so—or for both reasons. Their prospects should not be read off from pre-1914 European or Europe-derived experience. (The 1960s revealed more "Japanese" behavior in the developing world than Kuznets's analysis would suggest, for example, South Korea, Taiwan, etc.)

• There is some evidence of long swings in growth rates (over a span of some twenty years) in the few cases where continuous data exist for the period before 1914.

• Increases in productivity rather than in per capita man hours or material capital mainly account for the acceleration in product per capita during modern economic growth, stemming from the enlarging pool of science and technology.

• Conventionally calculated increases in product per capita overstate the growth rate for three reasons: some items listed as consumption should, in fact, be debited as costs of urban industrial life; other costs (for example, air pollution and noise) are not taken into account; education, to some extent hard to measure, is a capital cost as well as a form of consumption enriching the quality of life. (Kuznets's extended development of these points, pp. 75-98, is one of the best passages in the book.)

• Breaking out production and the working force under three gross headings (*I*, roughly, industry; *A*, roughly, agriculture; *S*, services), Kuznets emerges with findings most of which have been familiar since, at least, Colin Clark's pathfinding work in *The Conditions of Economic Progress* (London, 1941; London, 1951). During modern growth the *A* sector declines in its relative contribution to product and its call on the working force; the *I* sector rises; the *S* sector moves less consistently, showing a conspicuous rise in product share in certain of the richest countries. But the average lower productivity of the *S* than the *I* sector yields a marked increase in the proportion of the labor force in the *S* sector at high per capita product levels.

• The most important finding in this part of the work is that contemporary less developed countries show a less marked decline in the share

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of the working force in the *A* sector, with given increases in the *I* sector and per capita output, than historical data would suggest; whereas the post-1945 revolution in agricultural productivity in the developed countries yields more rapid decline than expected for the working force in agriculture. (Kuznets's data are from the 1950s. It is possible that the Green Revolution begun in the late 1960s may, in time, mitigate these findings; although they stem from the familiar concurrence of modern industrial enclaves in the midst of a still preponderantly traditional agriculture in some of the developing nations.)

These (and other) findings are buttressed at every point with massive statistical compilations in which one can observe deviations from average behavior, especially for the historical cases.

There is a curious asymmetry in Kuznets's analysis, stemming from his rejection of my distinction between the period of preconditions and take-off. In the historical cases, he accepts dates for the inception of "modern growth" which appear roughly identical with my take-off dates; although his dating is somewhat casual. In the contemporary world, he classifies nations solely on the basis of per capita product. In the 1950s (from which the bulk of his data derives) some were in what I would regard as the preconditions period, even if product per capita was quite high due to the availability of high value exports or other advantages; while some with low product per capita (for example, India) were clearly in a vigorous phase of industrialization.

This relates to Kuznets's fundamental unresolved dilemma: he defines modern growth in terms of the effective absorption of new technologies; he measures it in terms of product per capita, a quite different, if related matter.

Up to page 314, I read Professor Kuznets's work with my usual mixture of admiration and frustration: admiration for his stubborn and dedicated statistical virtuosity; frustration because his statistical categories are too broad to grip the bone structure and dynamics of the growth process. As I have said before, growth analysis at the level of aggregation used by Professor Kuznets is like playing the piano while wearing mittens.

But starting on page 314, he articulates in lucid terms his own frustration with the categories he has been using and tries to bring the story of modern growth back to its roots in the particular sectors and subsectors where new technologies actually enter the economy. He notes that his classification system fails to reveal the technologically new components and goes on:

> Since the high and accelerated rate of technological change is a major source of the high rates of growth of per capita product and productivity in modern times and is also responsible for striking shifts in production structure, it is

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> frustrating that the available sectoral classifications fail to separate new industries from old, and distinguish those affected by technological innovations (p. 315).

In truth, in ascending order, the *A*, *I*, and *S* sectors are hodgepodges of highly ambiguous meaning whose movements conceal much more than they illuminate about the growth process.

In the following 28 pages, Kuznets illustrates the need for greater disaggregation, if growth is to be linked to the coming in of new technologies. He discusses leading sectors, old and new, and sets up a model to illustrate the impact on aggregate growth rates of new rapid growth sectors and new products. It is a joy to be back with the author of *Secular Movements in Production and Prices* (Boston, 1930) and to note his sensitive appreciation of the innovational process, including his generous tribute to W. Rupert MacLaurin's minor classic, *Invention and Innovation in the Radio Industry* (New York, 1949).

But as I have pointed out on another occasion (Appendix B, *The Stages of Economic Growth* [Cambridge, 1971]), it is not easy to bridge the gap between a sound analysis of modern growth and the world of internationally comparable statistical data. We are all in the debt of Professor Kuznets for his immense effort over the years. There is something gallant as well as moving in his acknowledgment of how much of the gap still remains.

W. W. Rostow
University of Texas

Underdevelopment and Economic Nationalism in Southeast Asia *by Frank H. Golay, Ralph Anspach, M. Ruth Pfanner, and Eliezer B. Ayal. Ithaca, Cornell University Press, 1969.—xv, 494 pp. $15.00.*

This book documents the drive of Southeast Asian countries to match their political independence with economic sovereignty by decreasing the share of aliens in production and income. To focus discussion, the authors have coined the word "indigenism" as a comprehensive term to describe policies pursued.

Fundamental importance in the development of indigenism is accorded to the dualistic character of colonial economies where relatively modern trade-oriented enterprises existed in enclaves surrounded by a subsistence-oriented agricultural sector. The ownership of assets in the modern sector tended to be vested in aliens: Chinese, Indian, and Arab

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minorities as well as Westerners. Historically, this dominance appears to originate in superior business acumen and the propensity of minority groups to save. Such advantages were augmented by a high return to capital which utilized cheap labor and raw materials, while inexperience stifled indigenous entrepreneurship. In the eyes of Southeast Asians the situation was evidence that, at worst, discriminatory obstacles to their participation existed, or, at best, that colonial administrators failed to provide training and capital which would permit balanced racial participation. Given this setting, the writers analyze the initiative, persistence, and ingenuity evident in indigenization along three lines: 1) the comparative aspects of indigenism, 2) its relationship to economic development, and 3) lessons for the future.

The treatment of alien business varies: in Burma remnants of Western enterprise have disappeared and Asian alien enterprise is nearly extinct; in Indonesia, until the recent reliberalization, pressures on Western enterprise exceeded those against Asian aliens; in the Philippines a special position is established for American business as a *quid pro quo* for reciprocal concession, although Filipinism discriminates against Chinese minorities; in Thailand and Malaya pressures on Chinese exceed those on Westerners; in South Vietnam and Cambodia there appears no distinction in the treatment of Asian and Western alien business.

The assessment by development criteria is blurred. Within a static economic model of perfect competition, fixed technology and resources, it is argued that insofar as indigenous control is substituted for alien control, misallocation of resources results and potential output is sacrificed. However, through the more realistic dynamic model in which competition is imperfect, resources are accumulating, and technology is improving, no unambiguous conclusions are possible. When nationalist ownership, private or state, is substituted for aliens', the aggregate income often declines, but per capita income received by nationals usually rises. Yet income even to nationals may be sacrificed if returns to expropriated assets decline.

The future, according to the authors, will witness still more indigenism. Western participation might come through private foreign investment and governmental economic aid, although with limited leverage. Presumably indigenism will continue its association with chicanery as politicians and civil servants occasionally yield to temptations. Notwithstanding problems, it is emphasized that negative appraisal of nationalism in economic development has often been proved wrong.

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I wish to take exception with some points. The book seems to underrate the village population which undiscriminatingly "supports charismatic leaders" (p. 445). Indeed, as Sukarno's case shows, self-interest and community solidarity can invigorate the property-conscious peasants to the extent that charisma eventually dissipates. Occasionally, too, the discussion becomes beclouded due to the writers' failure to distinguish between political programs and their contents; for example, "Indonesian socialism" (p. 143) through cooperatives was advocated by Hatta, yet the nonspecialist reader would benefit if the private property philosophy and market orientation of his party were mentioned. And finally, there are the overlaps in the treatment which tend to plague joint authorship. But these incidental comments do not detract from an outstanding contribution.

JANOS HORVATH
Butler University

Types of Economic Theory, Vol. II: From Mercantilism to Institutionalism *by Wesley C. Mitchell, edited with an introduction by Joseph Dorfman. New York, Augustus M. Kelley Publishers, 1969.—xii, 875 pp. $15.00.*

In this, Mitchell's second volume, tracing theory (to quote his subtitle) "From Mercantilism to Institutionalism," we reach the core of his own interest—the place of institutions in economic analysis. There is the same ease of writing and the same relevant detail about the life and times of the economists reviewed as in Volume I. And we now find, beside a wide survey of Veblen's institutional approach, a specially valuable appreciation of the institutional elements in more orthodox economists. Veblen, we are told (p. 648), thought of calling his *Theory of Business Enterprise* "The Captains of Industry, A Romance"; and what he said satirically Alfred Marshall thought soberly. To both of them there was a human factor, an institution at work among the impersonal economic forces, at least on the supply side. Indeed, comparing Marshall's *Principles* and *Industry and Trade* with the modern textbook, one is struck by the disappearance of the institution in today's analysis. Most textbooks have among their early chapters one which describes the corporation. But no reference is usually made later to its influence, in contrast to that of the traditional sole-trader entrepreneur, paid directly by profit, either upon economic conduct or policy or performance.

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It is the textbook economists who fall into the error they commonly ascribe to institutionalists, of pure description for its own sake.

Mitchell is not concerned to advocate any one approach and is equally informative and illuminating on Schmoller and the historical school as on the theoretical aspects of Jevons, Menger, Walras, Gustav Cassel, von Wieser, and Schumpeter. His exposition is admirable (p. 165) of the symmetrical character of Marshall's theoretical framework in his Books III, IV, V, and VI, treating of the demand side, the supply side, value, and distribution.

As one of Marshall's pupils, I find admirable, too, Mitchell's reference to Marshall's immediate predecessors and contemporaries, such as Sidgwick, Edgeworth, Ashley, and Wickstead. Since Mitchell devotes considerable thought to welfare, more might have been said, perhaps, of Marshall's successor, A. C. Pigou. Certainly Pigou's 1912 *Wealth and Welfare* is much less vague than its third sentence, "welfare is undefinable," would lead us to suppose. The book and its subsequent enlargement made an impact in my undergraduate days and continue to influence English thought. J. A. Hobson is given pride of place in the chapter on welfare economics, and I remember being surprised at his inclusion when I attended Mitchell's course in 1917-18. But Mitchell brings out clearly that Hobson, like Veblen, paid attention to the new psychology growing up in his time and looked beyond purely economic motives and economic welfare. The contention of both of them that work was not always a real cost to be balanced against satisfaction in consumption certainly has application in modern cost-benefit analysis.

Mitchell has much to say, aside from Veblen, on American economists, in particular J. B. Clark, Frank Fetter, H. Davenport, B. M. Anderson, and John R. Commons; and European economists largely ignorant of the transatlantic schools of thought should find these chapters most useful in filling the knowledge gap.

A new feature in the present volume is the recording of questions by members of the class, which allow Mitchell to expound still further the more difficult points. He seizes upon this opportunity brilliantly.

Altogether, Dr. Joseph Dorfman, the editor, and Mr. Kelley, the publisher, are to be heartily congratulated on introducing and presenting this most scholarly work.

P. SARGANT FLORENCE
University of Birmingham
England

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Nationalism and Its Alternatives *by Karl W. Deutsch. New York, Alfred A. Knopf, 1969.—viii, 200 pp. $5.95.*

In 1963 Professor Deutsch chided certain authorities on Africa for treating tribal loyalty as "fixed and unlikely to change in any significant way during the next decades." Yet in this more recent work he notes that the late introduction and telescoping of those processes collectively known as modernization have thrust most Africans onto the political stage with "their old tribal loyalties still largely unchanged; and it becomes far more difficult to have them think of themselves as members of one new nation." Purely for rhetorical purposes he asks: "How are variegated tribal groups to become Tanzanians, Zambians, or Malavians in one generation?"

This alteration in Deutsch's viewpoint is evidently quite significant. The principal theme of his earlier work *Nationalism and Social Communication* had been that the assimilation process correlated with a number of quantifiable elements that accompany modernization. Heavily influenced by this thesis, writers on "nation building" have tended either to ignore the question of ethnic diversity that exists in approximately 90 per cent of all states or to assume that increases in social mobilization and communication within a multination-state will be accompanied by a corresponding diminution of ethnic identity in favor of a popular identification with the state. Only a few isolated voices have pointed out that the theory did not square with numerous illustrations of growing ethnic consciousness.

By contrast, Deutsch's most recent book devotes less than two complete pages to the assimilation process and treats it in terms of its "dimensions" rather than its "components." Moreover, the processes of assimilation and social mobilization are now treated as two causally isolated processes, whose primary importance to each other is in their chronological relationship. It is therefore difficult for this reviewer to perceive what predictive value the concepts of social communications and mobilization hold for the "nation builder." If the processes operate autonomously (or unpredictably) with respect to each other, then to say "if assimilation stays ahead of mobilization or keeps abreast of it . . . eventually everybody will be integrated into one people . . . [but] . . . where mobilization is fast and assimilation is slow, the opposite happens" is in fact to say very little. If assimilation is a lengthy process requiring generations and if Professor Deutsch is not recommending that the states of the Third World be immunized from modernization, then what is left is the conclusion that where assimilation has not yet been achieved, it is unlikely to be so—a conclusion definitely not in harmony

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with the theme of *Nationalism and Social Communication.*

Nationalism and Its Alternatives is seriously marred by a confusing use of key terms. What Deutsch calls a *people* is what Ernest Barker, Rupert Emerson, Carlton Hayes, and others called a *nation.* Deutsch here reserves the term *nation* for a people who have control of a state or, at least, an important measure of self-government. *A nation-state* is defined in the traditional manner as one which has become largely identical with one people. Despite these definitions, *nation* is often used interchangeably with *state,* and *nation-state* and *national state* are often used to refer to what in fact are multinational states. Thus Spain (actually a multinational state) is referred to as a *nation.* The Belgians (that is, the Flemish and Walloons who uncomfortably share the territory of Belgium) are likewise referred to as a *nation.* Such multinational states as Czechoslovakia, Romania, and Yugoslavia are referred to as *nation-states.* Most surprising, however, is that the central subject, *nationalism,* is nowhere defined. Sometimes it refers to identification with a state and other times to identification with an ethnic group.

There are two other aspects of the work that troubled this reviewer. One is Professor Deutsch's treatment of the states of Western Europe as fully assimilated entities, despite the fact that Belgium, France, Italy, Spain, Switzerland, and the United Kingdom have all been troubled by ethnic discord in recent years. The second objection is to Professor Deutsch's emphasis upon the material rewards and services that groups receive from association with the state as a major element in determining whether the state will be troubled by separatist tendencies. This position is seriously questioned by the prevalence of separatist attitudes among ethnic groups even while they were rapidly closing the economic and benefit gap between themselves and the state's leading ethnic group. Flemish and Slovaks are recent cases in point. Moreover, separatist desires have grown on the part of the Croats and Slovenes, despite the fact that both people are significantly better off than are the politically dominant Serbs. At least in some instances, then, it would appear that nothing secedes like success.

WALKER CONNOR
State University of New York at Brockport

Euratlantica: Changing Perspectives of the European Elites *by Daniel Lerner and Morton Gordon. Cambridge, Mass., The M.I.T. Press, 1969.—xii, 447 pp. $12.50.*

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Euratlantica is an interesting specimen of Ice Age ideology and as such it deserves attention and examination. The "data base" it purportedly rests upon was largely amassed through repeated TEEPS (short for The European Elite Panel Survey) that were conducted between 1954 and 1965 in England, France, Germany, and among "Eurocrats," i.e., high EC officials in Brussels, involving altogether "thousands of persons" (p. vii). As for the base itself, there is a considerable gap in *Euratlantica* between the furnished data and the formidable and top-heavy structure above them, which should not be interpreted to mean that more tables would have filled the empty space. As matters stand, the authors had trouble enough in fitting facts to theory, so much trouble, indeed, that a project that set out to "study our own time as 'history' " (p. 3) ends up as an anachronism. It is, nevertheless, to the authors' disregard of historical developments that we owe this vivid and inadvertent demonstration of (1) the postwar American power-nexus, (2) ideologies that helped to disguise and justify it, (3) some of the means employed to maintain it, and, last but not least, (4) the social scientists' intricate involvement that all this entailed.

The spiritual or psychic "dating" of the book is easy enough, although the fact that it was proofread as late as May 1969 is a source of genuine wonder. All the old cold war hocus-pocus is there: from the virtues of mass culture, pluralist politics, consensus in diversity, to the end (sometimes, but not consistently, the authors prefer to call it *passing*) of ideology, socialism, nationalism, parochialism, idealism, leftism, fanaticism, not to overlook communism.

The ostensible concerns of *Euratlantica* have to do with the growth of transatlantic and continental cooperation and integration and the consensual acceptance among the European elites of American "leadership" in this alliance. Indeed, the entire book optimistically bears witness to the past, present, and future success of this transatlantic system and its key institutions, most of all, NATO. Matters have developed so well, the authors are happy to report in the face of contrary evidence, that a "final conclusion is that the Euratlantic system is also beyond the point of no return" (p. 309)—so much so that the "Euratlantic ties cannot be severed by any European politicians . . . or any European policy . . . without arousing powerful opposition among the European elites" (p. 309).

Like any ideology worthy of the name, however, the book does not fail to provide us with evidence of its proper if unacknowledged subject matter: the "existential superpower of America" (p. 15) with its various military, economic, and political superinterests. In contrast to the largely idyllic text, two appended sample questionnaires are among the

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most coherent expressions of the prosaic pursuits of this "policy-related social research" (p. 75), a relatedness that is never elaborated on in the work. TEEPS simply monitored European elites' attitudes toward issues central to U.S. foreign policy, such as East-West relations, arms control and weapons systems, the Western alliance, NATO, etc. The reader dissatisfied with the results and analyses of a decade of political market research abroad as furnished by *Euratlantica* itself may find comfort in another annex, a "Select List of TEEPS Memos, Theses, Publications." In this list of close to seventy titles, almost all of which are unpublished, he discovers to his relief at least the private existence of such instrumental and unadorned subjects as: "A Cost/Benefit Analysis of French Elite Attitudinal Response to American Active and Passive Defense Systems," "Technical Reports" on the various national surveys, etc., etc.

In the power-nexus that is here inadvertently disclosed before the reader, the authors' own relation to power is one of simple, uninflected, and undifferentiated identity with the superpower, and represents by far the most fascinating aspect of the book. Enveloped in the oceanic feeling of America's superiority, and sharing, as it were, in her sovereignty, the authors survey the state of affairs among her European subjects and express satisfaction, approval, or scorn, as they see fit. The standards of judgment they employ are not only determined by the interests and material superiority of American power, but assume as well the moral goodness and superiority of this power, simply by virtue of the fact that it *is* power. The metamorphosis of might into right, the struggle of power for legitimacy, and the prudent decking out with ideologies of the pleasures and interests of power—all these processes in their various phases of development can be witnessed here. This demonstration would have been impossible without the authors' unqualified faith in the American supersystem, a system so exalted that it occupies the domain of the sacred in *Euratlantica*. It isn't even convincing as propaganda, let alone as ideology.

Frivolousness and blindness often go together with such overidentification. In style as well as substance, *Euratlantica* reveals these qualities in abundance. The Europeans will be happy to have it scientifically certified that after the war their nations "suddenly appeared to be small potatoes" (p. 215); de Gaulle has a narrow escape from being certified an "idiot" in print by leaving the obvious quotation from *Macbeth* unfinished; and reality breaks through the political science fiction in all those instances where one can read about America's "proconsulship" over European nations and how their prosperity and security are "subject to constraints imposed by their own relative weakness and by

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American world policy" (p. 52). Moreover, in order to secure their protection and advance their prosperity, Europeans had to adopt new psychological and sociopolitical attitudes and habits. "Americanization," the "American Nexus," and the "American Model," according to the authors, provided the common condition for this fundamental change in what with their customary elegance they call European "thoughtways" and "lifeways." Europeans and Americans alike will read with bated breath that many Europeans now perceive America "as the model of a new society—one whose power in the world derives from, and continually interacts with, the great strength of its domestic social order" (p. 38). *Sic!* And neither the Europeans, "awed with admiration" and prepared to "emulate" this American Model, nor the authors seem to find any flaws in it worth mentioning. In all fairness it must be added, however, that the book briefly mentions three social problems that beset the model: "These Europeans now believe that American society has developed a capability to absorb and adapt even the most serious of its internal problems . . ." (p. 38)—the population explosion. The characterization of the other two social problems reads: "The Americans, after decades of neglect, decided that their blighted cities had to be renewed and their degenerate race relations had to be elevated to a level consonant with the 'American dream' " (pp. 292 f.).

All in all, *Euratlantica* is an instructive book. It is alarming as well. Unintentionally it contributes to our understanding of how the platoons of experts become implicated in power and domination. Or is it that they are merely the emperor's tailors, sewing away at invisible clothes? Whatever may be the case, more and more Americans and people abroad are realizing that American power, military as well as economic, struts across the world scene, almost naked and bare of any decent drapery. And *Euratlantica* is an indispensable and indirect account of some aspects of this development.

GERTRUD LENZER
New York City

The Ideology of Fascism: The Rationale of Totalitarianism *by A. James Gregor. New York, The Free Press, 1969.—xv, 493 pp. $11.95.*

Fascism Today: A World Survey *by Angelo Del Boca and Mario Giovana, translated by R. H. Boothroyd. New York, Pantheon Books, 1969.—xi, 532 pp. $10.00.*

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Professor Gregor's book is a most useful contribution to the study of fascism. By contrast with much of the literature on Mussolini, Gregor takes fascist ideology as a subject appropriate for serious study and attempts to relate and analyze its development. The effort to prove ideology antecedent rather than posterior to action is not always successful, but the result is worth the labor devoted to it. The origins of Mussolini's thought are brought out by a careful examination of his voluminous pre-1914 writings. Gregor stresses the great influence of the Austrian Ludwig Gumplowicz whom Gregor considers to be the originator of most of the concepts of Mosca and Pareto. The evolution of Mussolini's thought toward an elitist concept of socialism is shown to parallel the development of Lenin's thought; Mussolini was only a little franker than Lenin. Subsequently, the experience of war seemed to show that analysis of society in terms of class was unrealistic; the self-identification of peoples in 1914 was by nation, not class. This led Mussolini to shift his perspectives to a nationalistic view of a world struggle between rich and proletarian nations and to a productionist rather than distributive view of socialism—trends that were later to be evident in the Soviet Union also.

Gregor holds that in Italy, as in Germany, the failure of traditional conservative elements to recognize the totalitarian implications of a clearly stated ideology led them to support a fascist movement that was in fact totally opposed to their conception of the state but would use them for its own purposes. The review of fascism in its years in power in Italy is not as successful as the discussion of its earlier evolution. The differences over Gentile's views, the role of racism in Italian fascism, and the socialistic experiments of the Italian Social Republic are discussed, but without the sureness of touch of the earlier chapters. It is at this stage that there has to be a closer coordination of theoretical analysis with historical inquiry. The relationship of theory to practice —which Gregor otherwise stresses—is examined least where it is most relevant. The concluding chapter on the totalitarian epoch, which returns to the similarities of one-party dictatorships in the twentieth century, is most thought provoking. It places fascism, bolshevism, and the new dictatorships of Africa into a single category of national developmental socialism and it should stimulate discussion in the scholarly community. Such discussion would have been facilitated if the author had not at some places allowed his enthusiasm for linguistic obscurity to becloud what is otherwise a fascinating book.

The survey of the two Italian journalists, originally published in Italy in 1965, is not likely to be very helpful to students of fascism. Written without any serious attempt at analysis or definition of fascism, filled

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with preposterously erroneous pseudo-history and general terminological confusion, this compendium of real or imaginary fascists and fascist movements of the postwar years is more useful for the student of a type of literature than the investigator of fascism. The book may serve as a sample of the journalist's "Grand Tour" that is more informative about the traveler's perception and milieu than about the places visited. Since the errors that leap to the eye are so numerous (Yugoslav Premier Stojadinovic was replaced by General Simovic lest he sell Yugoslavia to Mussolini, the *Heimwehr* were financed from Germany, the law professor Carl Schmitt is confused with the Minister of Economics Kurt Schmitt, etc.), one cannot avoid being skeptical about much of the information that is not so readily checked. If this is Tuesday, it must be the Belgian fascist party.

GERHARD L. WEINBERG
University of Michigan

Bayern in Umbruch. Die Revolution von 1918, ihre Voraussetzungen, ihr Verlauf und ihre Folgen *edited by Karl Bosl. Munich, Oldenbourg Verlag, 1969.—603 pp. DM 58.*

If a book were to be judged solely by the criterion of whether it met the expectations aroused by its title and preface, this volume of essays would deserve severe criticism. Yet, despite the obvious gap between intention and performance, it is rescued through the sheer competence displayed by several of the contributors. The fourteen authors represented are all alumni of Professor Bosl's seminar in Bavarian history at the University of Munich. To their *Doktorvater* they should be grateful for the opportunity to publish their findings in such a handsome format, but they are less well served by his prefatory remarks. While legitimately praising the variety of new materials and insights gathered by his students, Bosl has neglected to provide a guiding conception which would have permitted "dieses Teamwork" to attain more coherent results. In only three pages he attempts to elucidate two general propositions: that the impetus to revolution in 1918 did not develop overnight and that "revolutions can come from above or below." The first can hardly be considered novel and the second is a banality. Bosl alludes to a third hypothesis—concerning the "model character" of the Bavarian revolution—which might have been of considerable interest to historians and political scientists alike, but none of the authors makes any systematic effort to develop it.

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By default, then, it is to the subtitle that one must turn for a plausible scheme of organization. The tripartite division into preconditions, course, and consequences of the revolution at least suggests a useful chronological structure.

The preconditions receive the most and best analysis. More than half the essays focus primarily on the gathering difficulties of the Wittelsbach dynasty, the economic and demographic evolution of Bavaria before 1914, the political and social impact of the war years, and the radicalization of public opinion and political activity on the eve of the revolution. In this reviewer's judgment, the most original and important of these is the study of "Bureaucracy and Revolution" by Georg Kalmer, who is able to demonstrate how the alienation of the *Beamtenbewegung* from the old regime proved to be both symptomatic and critical in its collapse. Collectively the essays confirm that in Bavaria, as in Berlin, the major thrust of the revolution was not to abolish monarchy, nor was it merely a nervous reflex in a moment of military defeat. The crisis of 1918 was, above all, the result of a protracted social and economic development as well as the culmination of a previously thwarted reform movement. Bavaria was the scene of one of the many simultaneous attempts made throughout Central Europe to adjust a dilapidated political system to a transformed social reality.

The course of the revolution after November 1918 is touched upon in several articles, though treated thoroughly in none. This makes possible a detailed consideration of certain individual political figures (such as Kurt Eisner and the anarchists Gustav Landauer and Erich Mühsam), parliamentary factions (notably the Majority Socialists and the Liberals), and status or pressure groups (the bureaucracy, the Peasants' League). Unfortunately this procedure can only bewilder the uninitiated reader, especially when the same events are recited three or four times with no cross-reference or apparent connection. More importantly, perhaps, the concentration on some subjects is at the expense of others. The military establishment before and during the war, the soldiers' councils, and the Free Corps are scarcely mentioned. The position of the Church, the role of the clergy, and the activity of the (largely Roman Catholic) Bavarian People's party are neglected. The structure and function of the revolutionary councils of workers and peasants are specified only in passing. Little is said of revolutionary or reactionary agitation outside of Munich, nor is there any comparison of the pattern of events in southern Germany with Berlin or the rest of the Reich. The account is, in short, uncoordinated and something less than inclusive.

The consequences of the revolution nowhere figure prominently. The names of Ernst Niekisch, Ritter von Epp, Ernst Röhm, and Adolf Hit-

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ler do not appear in the index. The Communist leader Eugen Levine is mentioned only once. The few scattered references to the Bavarian Soviet Republic omit any evaluation of its importance; and this, in turn, precludes an investigation of the origins of the NSDAP and other political factions which thrived on Bavarian soil after the restoration of "order" in May 1919.

Many such shortcomings are doubtless unavoidable in a collection of essays. To his credit, Bosl is frank to concede that "the internal consistency of the individual contributions could not be entirely achieved." Yet he stops short of the conclusion that the collaboration of so many researchers—particularly when a precise and comprehensive overview is lacking—is not well suited to set a new standard of interpretation.

ALLAN MITCHELL
Smith College

French Socialists in Search of a Role, 1956-1967 *by Harvey G. Simmons. Ithaca, Cornell University Press, 1970.—xi, 313 pp. $9.75.*

Observers of the French Socialist party (SFIO) under the Fourth and Fifth Republics consistently have noted that, despite its elaborate ideological cover, it was a party much like the others. The substantial gap between Socialist aspirations and party performance has been explained as a consequence of the elementary fact that the party had to choose among unhappy alternatives or risk being isolated from the political system. Hence the SFIO, even when it directed the government, was not the pivot of the system of institutionalized compromise, but was trapped by it. Socialists nonetheless gave every indication of being willing victims, and participated without serious regret in their own undoing as a leftist alternative to political immobility under the Fourth Republic and to Gaullism under the Fifth. Rather than jeopardize their accomplishments, particularly the size, strength, and electoral power of the party, Socialists gave priority to keeping their own house in order and to avoiding adventure, whether this was an opening to the Left by accommodation with the Communists or a frankly "reformist" campaign to capture the presidency, as demonstrated by Defferre's abortive effort in 1965. In short, the Socialist party by 1969 was mired in immobility, dangerously vulnerable to the infirmities of a party in old age.

The merit of Professor Simmons's study, which covers the period from Mollet's lamentable premiership in 1956-57 to the disastrous show-

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ing of Defferre in the 1969 election, is to trace the response of Socialist leadership to a changing political situation. Indeed, Socialists did little more than respond, and only infrequently sought the initiative. Employing traditional materials, such as the *Populaire* and accounts of party congresses and the like, Simmons argues that the advent of the Fifth Republic offered the SFIO an opportunity to rethink its doctrine and strategy, but that the opportunity was essentially wasted. Instead, Mollet and the party wheelhorses were concerned chiefly with preserving and defending things as they were within the party. But herein lies the problem in Simmons's analysis: although his discussion of leadership attitudes, styles, and decisions is convincing, he tells us too little about the party itself. We learn that large federations were often decisive in votes at congresses, that the party held a large number of local political posts, and that many of its members were teachers. But we are not given any rigorous systematic analysis of the party's electoral geography or of its sociology, and this must be regarded as a weakness. If we accept his view that the leadership acted out of concern for the mood of the rank and file, surely we need to know more about those who were the party.

NATHANAEL GREENE
Wesleyan University

The French Presence in Cochinchina and Cambodia: Rule and Response, 1859-1905 *by Milton E. Osborne. Ithaca, N. Y., Cornell University Press, 1969.—xvi, 379 pp. $9.50.*

Professor Osborne utilizes French archival materials heretofore unexplored as well as sources available in both Saigon and Phnom Penh. His basic concern is to examine the so called "civilizing mission" which served as the perennial rationale for French imperialist endeavor, sometimes sincerely entertained, but always available as a kind of self-congratulatory excuse. He discriminates between the alternative policies of association (leaving social institutions, law, literature, and moral values undisturbed) and the arbitrary effort to promote the assimilation of French standards. The gallicization process encountered little resistance in half-Vietnamized Cochinchina, where mandarin officials withdrew in the 1860s. In Cambodia, by contrast, the French found it less easy or necessary to attack deeply rooted political and social institutions, so that the decadent monarchy managed to preserve some of its prerogatives.

French rule in Cochinchina focused on administrative and legal re-

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forms, along with the substitution of the convenient but culturally emasculated *Quoc-Ngu* script to replace the difficult but more meaningful Chinese literary medium. The efforts of several perceptive French colonial officials, particularly Philastre and Luro, to achieve a more genuine cultural rapport with Vietnamese law, society, and language ran aground amid the gloryhunters and champions of "civilization." Philastre was pushed aside, and Luro died in 1877. Pro-French Vietnamese collaborators, educated either in *Quoc-Ngu* or in French, in time formed a new non-mandarinate elite, including the wealthy landlord clique who profited from the French program of agricultural development. The resident peasantry in Cochinchina degenerated to the level of a rural proletariat, a factor which weighs heavily today in Saigon's efforts to compete with the land reforms sponsored by the Viet Cong. French efforts at assimilation in Cambodia were desultory in the face of King Norodom's persistent efforts to defend royal prerogatives and symbolism. The French tended to regard Cambodia as a museum piece, a kind of buffer against rival British influence in Siam and Malaya. For thirty years, they kept conveniently available a spare kinglet against the eventuality of Norodom's death or displacement. The substitute was not needed until 1905.

Professor Osborne's account is an exercise in political analysis rather than a history of French rule. He does not deal with contemporaneous developments in Europe or with concurrent events in Annam, Tonkin, or Laos, not to mention the competitive British presence in Malaya, Burma, or Siam. He makes no assessment of Louis Napoleon's role, the comparative influence of missionaries and naval officers, or the changing political climate in France during the eighties and nineties. A very valuable contribution, from the standpoint of future research in the field, is his twenty-page bibliography, which indicates the character and location of archival resources available in Paris, coupled with his sixty pages of annotated footnotes. Students of French Indochina are heavily in his debt.

JOHN F. CADY
Ohio University

Whitehall Diary, Vol. I: 1916-1925 *by Thomas Jones, edited by Keith Middlemas. London, Oxford University Press, 1969.— xxiv, 358 pp. $8.75.*

Before the repeal of the fifty-year rule, Thomas Jones's *A Diary with*

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Letters (Oxford, 1954) provided historians with a unique insider's view of the political personalities of the 1930s as seen by one who had served in the Cabinet Secretariat from its inception in 1916 to his move from Whitehall to the Pilgrim Trust in 1930. The 1916-30 period is now to be covered in three volumes, two of them chronological, a third entirely devoted to Irish matters.

There are a good many differences in scope and approach between the earlier diary and the later, which are ably analyzed by Middlemas in his introduction to this volume. The new volumes cannot have quite the impact of the 1954 book, given the opening of various other official and unofficial papers since then. But Middlemas does not greatly exaggerate when he argues that Jones's account will always "give an unparalleled insight into the years after the First World War," no matter how industriously the many other seams are mined.

Tom Jones did not have it in him to be "the Greville of our age," as J. L. Garvin once suggested (p. 268), but he did possess many of the attributes of the born political diarist: he was a shrewd professional busybody, superbly placed to observe policy making at the top, yet having a wide range of acquaintances and sympathies to protect him from the peculiar parochialism that is endemic to all inner circles. The 1916-25 volume contains few surprises, but much valuable corroboration, supplementation, and added insight.

At times, the full flavor of Cabinet debate is captured to a degree that no minutes can ever reveal, and with a verisimilitude only to be obtained at the time, without passing through the powerfully distorting lenses of later recollection. But the principal characters emerge pretty much as we already know them. If anything, the familiar contrasts are sharpened, not altered. Churchill, for all his romanticism, his mercurial temperament, and his shortcomings as a dependable colleague, exhibits once again his grasp of many crucial realities—such as the need for German recovery as a prerequisite for a healthy Europe (pp. 116-17). His anguished comment on Anglo-French differences over reparations and the Ruhr in 1921 could stand as a monument to many interwar failures: "I fear that we shall be left without our policy and not completely with theirs" (p. 156). Baldwin fares poorly. Jones comments in 1923: "Bonar Law would do as much work in an hour as S.B. in four or five. I am never sure whether the P.M. is thinking at all or simply wool-gathering" (p. 256). Cabinet-making in 1924, he says to Jones, "What I want from you is a Minister of Education: have we any man in the Party who takes any interest in Education?" Even the famous Baldwin honesty comes through as strongly tinged at times with self-delusion and a kind of moral narcissism.

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The editing is generally skillful, perhaps a bit spare—the reader anxious to follow a subject further will find little help in the footnotes, together with a good many undocumented assertions. There are, inevitably, a few errors: for example, the Hadow Report of 1926 grew out of, rather than inspired the "significant advances in Education" made by the first Labour government (p. 262); Lord Reading was viceroy of India, not secretary of state (p. 272, n.3).

But the book is Tom Jones's, and the editor commendably leaves it that way; it will be among the few genuinely indispensable items on any bibliography of British twentieth-century political history.

RICHARD W. LYMAN
Stanford University

Egypt and Cromer: A Study in Anglo-Egyptian Relations *by Afaf Lutfi al-Sayyid. New York, Frederick A. Praeger, 1969.—xiii, 235 pp. $8.50.*

"Egypt will have come of age when it erects a monument to Lord Cromer," a famous Egyptian is reputed once to have said. While no monument of Lord Cromer has yet been erected in Egypt itself, an important first step has now been taken by a most remarkable young Egyptian woman scholar, Dr. Afaf Lutfi al-Sayyid.

A product of the new generation in Egypt, Dr. al-Sayyid has nevertheless eschewed all temptation to twist historical facts to support historical opinions now popular in Egypt. Instead, she has reexamined the sources and has offered an interpretation which reflects an Egyptian viewpoint without doing violence to the historical evidence at her disposal. Hopefully her work is, or will be, available also in Arabic and will stimulate other Egyptian historians to follow the lead so courageously taken by Dr. al-Sayyid.

The first six chapters of the volume set the well known story of Egypt just before and after the British occupation within an Egyptian context, a context all too often ignored or romanticized by European historians of the period. Yet the inside story of the Urabi revolt and the intellectual background of the period elude Dr. al-Sayyid as they have everyone else. The problem stems probably from the fact that no systematic study of the Egyptian archives has yet been carried out, although Dr. al-Sayyid was in a better position than most to engage in extensive research in the Egyptian archives. In her defense, it should be said that it is unlikely that the Egyptian archives would have in any

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event produced the kind of materials needed to recapture the mood of the people of Egypt as it truly was and not as one supposes it should have been.

The lack of private papers by leading non-European personalities in Egypt puts an Egyptian historian at a great disadvantage. Not only are European political dispatches often very revealing of personalities as well as of events, but the European historian often has access to an occasional note or personal comment, an intimate letter between prominent actors on the historical stage, evidence that gives important insights and facilitates the reconstruction of mood as well as a recital of facts.

The consequence of this lack of personalized materials for the earlier period has forced Dr. al-Sayyid, with some exceptions, to rely heavily on European materials. To the degree that she views these materials from an Egyptian vantage point, she introduces some interesting interpretations. Because she is so fair-minded and so disciplined an historian, however, she does not permit her emotions to distort the evidence she employs. The result is, consequently, that except in detail and outlook, her work and those of fair-minded European scholars do not differ substantially.

The situation changes, however, in the last three chapters of her book where the availability of a whole new body of materials is clearly reflected. From the period of Abbas Hilmi II and the emergence of the nationalist movements, Dr. al-Sayyid can turn to memoirs, personal interviews, Egyptian periodicals, and her own close personal knowledge of Lutfi al-Sayyid. In the latter part of her book, Dr. al-Sayyid writes with a sureness of touch and with a splendid historical objectivity. She unravels the complex tangle of personalities, personal motivations, ideas, and political forces both with taste and good judgment.

The reader feels a new sense of pain and moral indignation at the notorious Dinshwai incident and is less willing than heretofore to accept British justification for the excessive punishment imposed. Unexpectedly, Mustafa Kamil commands greater understanding and sympathy, despite the author's frankness in her assessment of his shortcomings. The fact that Lutfi al-Sayyid recognized that social reform must accompany national liberation serves as testimony to his political wisdom and foresight. Finally, the judgment that "in spite of his shortcomings Cromer was a worthy opponent" (p. 197), does credit to the author's historical sense as well as to the patriotic zeal of the generation of Egyptians who fought the great proconsul so gallantly.

Nevertheless, the reader is left with a sense of bitter regret that the great liberal spirit that imbued Muhammad Abduh, Saad Zaghlul, Lutfi al-Sayyid, and others was eventually crushed. As the author states so

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forcefully in her epilogue, "the liberal experiment failed, for if Egyptian politics had a pattern, they lacked a system . . ." (pp. 207-08). Egypt achieved self-rule in 1952, but the leadership came not from Egypt's liberal stream shaped in the age of confrontation between the liberal Lord Cromer and the early liberal Egyptian nationalists, but from the authoritarian stream that has played such a pervasive role in the Egyptian historical experience.

HELEN ANNE B. RIVLIN
State University of New York at Binghamton

Soviet Perspectives on International Relations, 1956-1967 *by William Zimmerman. Princeton, Princeton University Press, 1969.—viii, 336 pp. $9.50.*

For a Western observer who does not read Russian and must therefore get his knowledge of the Soviet theoretical approach to international relations from English-language publications, such as *International Affairs,* what stands out in that literature is its dogmatic character, to the point of a mechanical ritualization, on the one hand, and its extensive use of Western original sources, on the other. What is dogmatically asserted, buttressed by ample quotations from Marx and Lenin, thus acquires the appearance of empirically proven truth. Since the assumptions and conclusions of this kind of literature are *a priori* predictable, its perusal is utterly unrewarding.

It is the great merit of Professor Zimmerman's book to acquaint us with a whole body of scholarly literature which is not accessible to those of us who do not read Russian and which is quite different and much more valuable than what we would have expected from reading what the Soviet Union makes accessible to us in the English language. Who would have expected that a book dealing with the United States could be published in the Soviet Union in 1964 of whose 1,585 footnotes only one refers to Lenin, one to Marx alone, and one to Marx and Engels? Behind the ideological facade of Marxist-Leninist orthodoxy, there is obviously considerable intellectual life to which that orthodoxy is largely irrelevant. That is, of course, the same conclusion other authors, such as Richard Loewenthal, have arrived at who have investigated the practice of Soviet foreign policy.

Professor Zimmerman shows clearly that the emergence of a Soviet science of international relations worthy of the name was predicated upon the disappearance of Stalin and Stalinism: "Thus international rela-

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tions studies were essentially starting from scratch in 1956. The major impetus to the legitimation of international relations as an area of inquiry came at the 20th Party Congress," which engaged in an explicit criticism of Soviet social sciences. Under the impact of that criticism a great number of excellent books and articles has appeared in the Soviet Union, dealing with a variety of topics, which the author classifies under the headings of "The Actors," "The Hierarchy," "The Distribution of Power," "United States Foreign Policy from the Soviet Perspective," "The Balance of Power as System and Policy," "Post-Imperialism and the Transformation of Soviet Foreign Policy."

Thus, this book not only enlarges our factual knowledge in an important academic field, but also sheds an illuminating light upon the peculiar relations between the official ideology and the substance of scholarly thought carried on behind it.

HANS J. MORGENTHAU
City University of New York

Communism and the Yugoslav National Question *by Paul Shoup. New York, Columbia University Press, 1968.—xii, 308 pp. $9.50.*

Yugoslavia: The New Class in Crisis *by Nenad D. Popovic. Syracuse, N.Y., Syracuse University Press, 1968.—xv, 240 pp. $7.00.*

The two books under review have very little in common; the first is a thorough and rather positive analysis of Communist policy toward the problem of nationalities; the latter is a thoroughly critical analysis of the Yugoslav political system.

Professor Shoup's work is a careful, scholarly study of the complex problem of the national question, the solution of which has baffled almost every multinational state regardless of its form of government. It has defied the efforts of the Yugoslav Communist regime, too. In the interwar period, the CPY changed its position on nationalities, as other Communist parties did elsewhere in Central and Eastern Europe, according to the opinion of the prevailing intraparty faction and to the opportunistic instructions from the Comintern. The war presented Yugoslav Communists with a formidable problem in a country that was being torn to shreds by nationalist hatreds. To their lasting credit, they coined the slogan *bratstvo i jedinstvo* (brotherhood and unity);

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and when Yugoslavia was saved as a state from the danger of dismemberment or another centralized rule from Belgrade, they established at least a promising foundation to a just solution of the national question by turning the country into a federation. The party, however, maintained strict control over every aspect of life in every region of the country, and any polemic over the problem of nationalities was eliminated from the mass media. On the surface, the problem appeared to have been solved within the context of a multinational party rule.

However, with the process of several constitutional reforms, numerous administrative and economic measures of decentralization, and political and cultural liberalization, the national question has once again pushed itself to the forefront of Yugoslav public life. Emotions run high, overshadowing rational arguments particularly in the area of economic planning, and it is most disconcerting to see that it is no longer the old "bourgeois" society of the prewar period but the new "partyzan" generation, occupying not infrequently key positions, which has been infected by the germ of nationalistic particularism. This discouraging experience simply proves that mistrust and irrational sentiments have deep historical roots which cannot be eradicated by the valiant efforts of policy-makers—a phenomenon which the author does not appear to appreciate fully.

Shoup's book is, nevertheless, a first-class work, a product of scholarship in depth, based on a study of original materials and turned into a highly readable result. It is a major contribution to the study of not only contemporary Yugoslavia but also of the ever-vexing problem of the national question in all, particularly Communist, multinational states.

Professor Popovic's work mentions the national question only in passing (pp. 186-88) as a factor affecting the political and economic structure of Yugoslav affairs of state, which is the focus of his study. Borrowing from Milovan Djilas's book title, *The New Class*, he presents the thesis that the new class rules and lives above the law and is alienated not only from the society but also from the party itself. The new class perpetuates its exclusive position by considering every reform in the light of its own political power, and it conducts a foreign policy dedicated to the Communist cause from the same perspective, to assure the regime of ultimate national and international security. It applies the same approach to the problems of economic development, which suffers from fundamental contradictory interests between the new class and wage earners. Even the workers' councils, which attracted much attention in the West and in some quarters in the East, are condemned by the author as a smokescreen to cover the real motives and goals of the new class.

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Popovic, who was a high official in the Yugoslav government and is a competent economist, offers plausible insights into the economic picture of the country, but his original thesis about the new class does not carry full conviction in the other chapters of the book. Emphasizing structures and institutions, he appears to overlook the meat of the matter, seen—as it should be—in the light of political, economic, social, and cultural achievements by Tito's government and party over the period of one-quarter of a century.

JOSEF KORBEL
University of Denver

Present At The Creation: My Years in the State Department *by Dean Acheson. New York, W. W. Norton & Co., Inc., 1969.—xvii, 798 pp. $15.00.*

Dean Acheson describes his memoirs of State Department service from 1941 to 1953 as "a tale of large conceptions, great achievements and some failures, the product of enormous will and effort. Its hero is the American people, led by two men of rare quality, President Truman and General Marshall" (p. xvii). For his story, Mr. Acheson disclaims the virtues of detachment and objectivity. His stated aim was to write an account which would dispel the prevailing "mood of depression, disillusion and withdrawal from the efforts to affect the world around us" (p. xvii). *Present At The Creation* succeeds superbly in conveying its grand message. But its ability to reinvigorate the flagging national mood remains to be proven.

Present At The Creation is an unashamedly partisan, one-sided book. It rarely displays a doubt about who the heroes and villains of history are and what policies deserve the accolade or condemnation of Americans: "Vishinsky was a natural blackguard," Jan Masaryk "a charming, gay figure" (pp. 52, 77); Russian policy on Germany was "clumsy" and "insensitive," while the U.S. acted "with dignity and logic" (pp. 274, 76). Yet, despite its dogmatism, the book commands respect because even villains, like General MacArthur, are credited with their strengths and heroes condemned for their weaknesses. MacArthur was "bright" although his "incredibly bad judgment" did inestimable damage to the U.S. (p. 526). Truman, forthright and perceptive, was at times "naive," with a mind "not so quick as his tongue" (p. 192). These judgments are made by a man whose reputation is buttressed by his vast experience in foreign policy during a trail-blazing era in U.S. history.

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It was during Acheson's period of service as assistant secretary, under secretary, and secretary of state that the U.S. embarked on such policies as the Marshall Plan, Point IV, the Truman Doctrine, the Korean war, and substantial aid for the French in Indochina. Acheson may overstate the case when he likens the post-World War II years to the chaos preceding Genesis—years "to create half a world, a free half . . . without blowing the whole to pieces in the process" (p. xvii). But few, regardless of their evaluation of the achievements of the period, would deny that the Truman policies displayed bold innovations which thrust U.S. policy in new directions.

There is much solid conventional wisdom in Acheson's book about misperceptions of reality by people of good will, deceived by wishful thinking. Thus, in spite of the Marshall Plan's success, he does not consider foreign aid a panacea. He warns that it will fail in countries where the will and capacity to use it fully are lacking. He advocates neutralizing the political importance of the Middle East by technical innovations which will diminish the world's petroleum needs. He stresses the imperative of using power unhesitatingly as a deterrent to aggression. He urges the building of alliances among like-minded governments, because he views international organizations, including the U.N., as slender reeds on which to rest the fate of peace in a divided world.

The book contains a wealth of well drawn sketches of American foreign-policy making during the forties and fifties. Many of them reveal Mr. Acheson's abiding distrust of the Soviet Union and its appetite for aggression at the expense of the United States. Unfortunately, so many incidents are presented in the disarray termed "chronological order" that the broad sweep of policy becomes obscured.

Among the most rewarding portions of the book are its vignettes of the world's leaders and their lieutenants. It is a veritable *Who's Who* of the foreign policy Greats of the Western world, written with flourish and polish, acidly but not maliciously, admiring but not fawning. Through Acheson's eyes we see Truman, "the captain with the mighty heart" (p. v); Nehru, "one of the most difficult men" to deal with (p. 336); and the slow-moving, inscrutable Adenauer. We meet Clement Atlee, "persistently depressing, his thought . . . a long withdrawing melancholy sigh" (p. 478), and the venerated Churchill, whose "supreme art and deliberate policy . . . transformed courage, right decisions, magnificent oratory into something different and superlative" (p. 595).

To single out any particular policy narration for special comment would merely impose the reviewer's priorities of interest on the reader. Acheson seems to have missed nothing in his memoirs. To the tales of events he has added perceptive analyses of the agents and agencies of

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foreign policy formulation—the operations of the State Department, the style of the president and his aides, and the influence of Congress on the foreign policy process.

If one must fault Acheson on any score, it should probably be on his supreme confidence that all the policy options of the Truman administration were wisely chosen and yielded the best possible results. Failures sprang from tragic circumstances and could not have been avoided by any feasible policy choice. Achievements always fall "short of both hope and need" (p. 725). Nor does it seem completely fair to blame unfavorable developments in China, Indochina, and elsewhere wholly on policy architects outside the State Department. Chiang Kai-shek's incompetence and folly and French intransigence do not fully explain American policy failures in these areas.

Weary from reading nearly 800 packed pages, one might carp that Acheson has told too much. But then a twelve-year span of foreign policy was never meant to be compressed into an evening's easy reading. For the student who is willing to sample the Acheson volume in measured doses, taking his pick from those portions which interest him most, *Present At The Creation* will be a fascinating and valuable source book. When he finally finishes the lengthy tome, the reader will surely concur in Acheson's admonition that "perseverance in good policies is the only avenue to success" (p. 728).

DORIS A. GRABER
University of Illinois, Chicago

High on Foggy Bottom: An Outsider's Inside View of the Government *by Charles Frankel. New York, Harper and Row, 1969. —xii, 240 pp. $6.95.*

This outsider's "inside view of government" by an uncommon professor of political philosophy who served two and one-half years as assistant secretary of state under Lyndon Johnson and Dean Rusk is crammed with fascinating anecdotes and insights that convey a more realistic feel of how the government really works than can be gained from most textbooks on the subject. Although the locus of Charles Frankel's experience was in the Department of State—in particular the Bureau of Educational and Cultural Affairs (known as CU)—his observations extend to the whole government.

This book is a fitting sequel to *The Neglected Aspect of Foreign Affairs*, a critical examination of America's hodge-podge of international

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educational and cultural programs written by the author just before being called to Washington. It was a rare opportunity for a social philosopher to put his theories into practice, but the fates ruled otherwise and he finally departed in a mood of defeat.

Frankel, like some of his predecessors, took the CU baton in hand with three clear objectives in view: (1) to make the government's motley programs in this area more coherent and effective; (2) to lift these unique tools of peaceful, long-range foreign relations to the level of importance they deserved; (3) to liberate their overseas management from the propaganda grasp of the United States Information Agency (a bureaucratic miscarriage held over from John Foster Dulles). By his own standards, he largely failed on all three scores. Why so?

The chief devils of the piece, refreshingly enough, were not Johnson and Rusk (whom the author views more with pity than anger); they were the Viet Nam war, State Department's incredibly ingrown "system," and the legendary Congressman John Rooney of Brooklyn, chairman of the House Appropriations subcommittee, who, many old hands contend, exerts as much influence on the conduct of U.S. foreign relations as the secretary of state or the president. Considering the enormous outpouring of books on U.S. foreign relations each year, it is strange that no enterprising scholar has seen fit to examine Mr. Rooney's extraordinary and little known role.

Unlike many recent exiles and escapees from Washington, Frankel is neither out for blood nor out to reveal dramatic "inside stories" about who did what to whom. His modest purpose, in which he succeeds admirably, is to portray as vividly as he can what day-to-day life in government looks like to an interloping high-level policy maker and administrator.

His Columbia students and many others who have visualized the foreign policy establishment as an efficient group of crafty plotters will get from this book a quite different and more realistic picture that is even more disturbing. Frankel depicts the machinery of the American government as a vast bureaucracy trapped in its own myths and inertia, able to hear only its own voice most of the time, and thus incapable of responding adequately to the will of its constituents or to the rapidly changing world around it.

In the final chapter, summoning his innate faith and optimism, Frankel saves his readers from total despair with some positive suggestions for a way out. Briefly, he urges: (1) a heightened circulation of fresh people into government and of tired ones out; (2) far-reaching congressional reform, beginning with the abolition of the seniority system; (3) the regionalization of many functions now centered in Washington

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so as to bring government closer to the governed and make it more flexible and venturesome; and (4) the transfer of many operational tasks now performed by overstuffed Washington bureaucracies (such as running the social security machinery and international programs of educational and cultural affairs) to quasi-public and private organizations.

This may be dismissed by many so called "realists" as the impractical formula of an academic. But as Frankel observes with quiet restraint, "If all this seems difficult, it can only be said that it is not nearly as difficult as bringing the American government, as it is presently constituted and as it presently conceives its functions, to a level of good performance." This reviewer, who shares both Frankel's faith and his scars of congressional and bureaucratic battles, can only say, "Amen."

PHILIP H. COOMBS
Essex, Connecticut

The American Occupation of Germany: Politics and the Military, 1945-1949 *by John Gimbel. Stanford, Calif., Stanford University Press, 1968.—xiv, 335 pp. $8.75.*

The military-political occupation regime has become a frequent instrument of policy in this century, but one misses an adequate elucidation of its characteristic features. One reason for this is, undoubtedly, the inaccessibility of archival material. There are also, however, conceptual problems. Occupation politics exists at a number of different levels. It includes episodes in the domestic politics of at least two states; it involves rapid changes of status between occupier and occupied; it occurs in a period especially conducive to (self-) deceptive mythologizing; finally, it involves a peculiar, dependent political relationship.

To deal with these factors is task enough; the difficulties are compounded when the occupation is quadripartite. One must then add relations between the occupying powers to the above questions. In such circumstances, it is not surprising that there are few satisfactory studies of the four-power occupation of Germany after 1945.

One consequence of this situation has been that the best studies thus far have been historical ones; merely to set the record straight is a worthwhile goal. Among American historians of the occupation of Germany, John Gimbel holds a prominent position. His previous case study of the American occupation of Marburg was a valuable contribution to detailed knowledge of occupation relations.

In the work under review, Gimbel has done an outstanding job of ex-

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amining and assimilating a large body of American archival material relating to the detailed implementation of policy in the field and its effects on larger policy making, and of showing how policy derives from a mixture of motives, rather than from a single policy line. (But it is perhaps a weakness of Gimbel's approach that he underestimates the effect of a consistent policy approach by field-level personnel.)

Gimbel usefully challenges some traditional interpretations of the history of this period. Going beyond the work of F. Roy Willis, he shows how many American initiatives were designed to overcome French objections, but how frank acknowledgment of this situation was replaced, for cold war reasons, by attacks on Soviet policy. In an all-too-brief discussion (pp. 202-04), he considers how Soviet and American policies in the spring of 1948 may have been in fact conditioned by a false perception of the other's long-range consistency. The author argues less convincingly that from pre-Potsdam 1945, through the Conference, and during the entire period, the American occupation regime followed a consistent policy set out in JCS Directive 1067. Briefly, the fact that Clay and Murphy may have sought to pursue a consistent policy under any and all directives does not demonstrate that various instructions did not have diverse effects.

In fact, many of Gimbel's insights are undercut by his excessive focus on details. While stressing that U.S. occupation policy was guided more by conceptions of American interests than by goals of transforming Germany, he slights the effects of such conceptions on every day occupation affairs. Gimbel properly stresses the role of France in Western policy making, but his sparse discussion of Soviet policy has a distorting effect on his account. Perhaps these and similar complaints are the result of different disciplinary perspectives. As a historian, Gimbel is concerned to substitute accurate narrative for vague or self-serving "grand" explanations. A political scientist misses a working model of occupation policies which would place the American experience in a wider perspective.

HENRY KRISCH
University of Connecticut

The New Deal and the States: Federalism in Transition *by James T. Patterson. Princeton, N.J., Princeton University Press, 1969. —viii, 226 pp. $6.50.*

Basically, this book is a study of the New Deal's influence on the states. More interpretive than analytical or detailed, it contains a balanced sum-

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mary of state political, legislative, administrative, and financial developments during the Depression, with illustrative accounts of those states from which most may be learned about the impact of social change and national policy, particularly social policy. In this way the author handles the problem of coping with 48 separate entities. And, judiciously, he leaves his verdict on state performance in doubt until the end.

Patterson's ultimate evaluation of the states, both in the twenties (reviewed in his first chapter) and thirties, is negative. He begins the book by quoting Justice Brandeis and Governor Roosevelt on the virtues of decentralized federalism; but he finds little in the twenties and less in the states' initial response to the Depression to support these views. Patterson concentrates on social policy and governmental reform. He concludes that New Deal welfare policies worked inequities within states while grant-in-aid patterns were inequitable between states. Social and reform legislation was negligible in many states and unimpressive in most. Generally, state tax legislation in the thirties was badly regressive. There was a pronounced alteration of national-state relations in the grant programs, and there was uneven state progress in social policy and administrative performance: How much these stemmed from inevitable centralization, how much from New Deal pressures, and how much from state initiatives is hard for Patterson to say.

Patterson concludes that "The New Deal produced neither federal dictation, a completely cooperative federalism, nor a new state progressivism. Instead, it helped create a rather flat mixture of achievement, mediocrity, and confusion. For all the supposed power of the New Deal, it was unable to impose all its guidelines on the autonomous 48 states" (p. 202). Why was this? Patterson comments particularly on public cultures and institutions, fiscal capacities, and political patterns. For the most part the states inherited laissez-faire ideologies and primitive administrative systems; and reform did not go far enough to change these fundamentally—the more so as time ran out on the New Deal before its programs took hold firmly in the states. Few states contained the fiscal resources to support progressive social policies: here the author seems a bit uncritically influenced by political scientists in whose recent statistical studies fiscal bases outweigh political factors as explanations of state policy. Patterson mentions no methodological doubts about these studies; on the other hand, he suggests that electorally competitive states tended to be most progressive, though he hardly isolates competition as a cause. Further, he finds both Republican resistance and Democratic factionalism solid obstacles to reform: his discussions of national-state political relations are among the best in the book, indicating that po-

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litical centralization was impeded by a federal party structure and further state party fragmentation, some of which reflected a diversity of policy concerns but much of which was personal politics. Politics, administration, and finance remain inextricably mixed in the explanation.

In sum, state governments were generally conservative and unresponsive during the Depression. The New Deal clearly pitted governmental initiative against states' rights, redefining the issue without resolving it. Where states were progressive, or even distinctively conservative, this was traceable to strong governors. While negative toward the states, these findings are not *necessarily* conclusive on a future that may provide revenue-sharing, reform of state institutions, and better federal management. They do show the problems of the states' capacity for social policy in modern society to be serious and enduring. Patterson's history contributes importantly to our perspective on these problems.

CHARLES E. GILBERT
Swarthmore College

The Politics of Provincialism: The Democratic Party in Transition, 1918-1932 *by David Burner. New York, Alfred A. Knopf, 1968.—xiii, 293 pp. $6.95.*

After Harding's crushing victory in 1920 and the Democratic debacle four years later, many were ready to bury a dying party. And yet, after the defeat of Al Smith in 1928, the Democrats were to lose only two of nine presidential contests and one of 19 congressional elections. The intriguing explanations for this remarkable transition is vivdly portrayed in this significant, scholarly contribution. Delving into hundreds of manuscript collections, doctoral dissertations, scholarly articles, regional newspapers, and Columbia University's Oral History Collection, David Burner has evolved new insights, shattered old shibboleths, and demonstrated that a scholarly historian can be a good political scientist.

Typically, the author insists that since the Democrats lost the 1918 congressional elections because of sectional issues, the results cannot be viewed as a repudiation of Wilson's plea for a vote of confidence. He demonstrates that by 1922, the big cities had gone Democratic and generally remained so in congressional elections. Although many industrial areas between Boston and Chicago went to Smith in 1928, it was Robert M. LaFollette who first won large numbers of workingmen from the GOP. His Progressive party (1924) functioned for many voters as a way station between the Republican and Democratic parties.

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During this period of Republican ascendancy, as John D. Hicks contends, the national Democratic party reshaped itself from a divided, urban-rural conglomeration to one with an urban-immigrant orientation. Not since 1892 had Democrats been able to secure a majority vote in the ten largest cities; not, that is, until Al Smith in 1928. Though he helped mold "the city immigrant Democracy into a force of unprecedented cohesion," a tactless and provincial Smith exacerbated instead of calmed, and failed to reach out to a large sector of voters. But equally important, he was confronted with an able Republican who, in many ways, had greater progressive appeal. Despite Hoover's characterization of Smith's program as "State Socialism," the author is correct in portraying the Democratic campaign and candidate as basically conservative.

Acknowledging the influence of Republican prosperity in Smith's defeat, the author challenges the contentions of earlier commentators like Ogburn and Talbot, Ruth Silva, Richard A. Watson, Robert M. Miller, and others that Smith's Catholicism was not too great a liability or that his stand on prohibition insured the greater loss of votes. Recent studies disclosed that in 1960, as many as 40 per cent of Democratic Protestant voters cast ballots for John F. Kennedy's opponent. How much worse, three decades earlier! In contrast to Kennedy, Smith neglected to face squarely the religious issue in a consistent, dispassionate manner.

Through statistical analysis of key voting districts, the author demonstrates that, in defeat, the "Happy Warrior" instilled new life into the Democratic party in the Far West and farm belt, but particularly in urban areas. In the process, he helped reconstruct the party and liberate it from Bryan and the South. Thus, Burner has documented Samuel Lubell's impressionism.

In the face of the Great Depression, Democrats in Congress followed a conservative course. It was Governor Franklin D. Roosevelt of New York, however, who formulated a progressive response and whose harmonizing influence united the extremes of the party into a cohesive and winning force.

Political scientists and historians are deeply indebted to this very promising scholar.

BERNARD BELLUSH
The City College of the City University of New York

We Shall Be All *by Melvyn Dubofsky. Chicago, Quadrangle Books, 1969.—xviii, 557 pp. $12.50.*

Until recently, historians took the Industrial Workers of the World rather

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lightly. They valued it as a source of American folklore. They were attentive to its fate as victim of the most horrendous instance of political repression in our history. But, considered as a radical movement, the IWW itself seemed of slight significance. Now we are having second thoughts. Perhaps there is to be found in the IWW a characteristic pattern of American radicalism—a key to what is happening today among blacks, students, and peace groups. The possibility has revived scholarly interest in the Wobblies and led to a spate of new books. In Melvyn Dubofsky's *We Shall Be All* we have a major history that does the guild of labor historians proud.

The point needs to be emphasized. From the standpoint of historical craftsmanship, scholarship in this field has not been very notable. Now at last American labor history is coming into its own, and Dubofsky's book is a prime example of the high order of current work in the field. His research is exemplary. Given the destruction of IWW records by the federal government, Dubofsky has done a remarkable job of finding alternative sources of information. The book is, in addition, of absorbing interest; no leaden sentences or turgid chapters here. Dubofsky has a gift for incisiveness: complex events—the repression of the IWW, say—take comprehensible form in his hands. There is, too, a shrewd historical mind at work. At every controversial point, Dubofsky lays down the evidence and draws unhesitating conclusions—with, it must be said, a somewhat deplorable glee for putting down other interpretations. Finally, Dubofsky avoids a characteristic flaw of American labor history. He consistently relates the history of the IWW to the larger setting. We can at last understand the social-economic sources out of which it arose, the reasons for its brief success, and the forces that killed it. This is, as another reviewer has said, a fine, fat book.

What emerges from it is an intimation of the radicalism of the dispossessed in America. Dubofsky does not quite make explicit the implications of his analysis—somehow a "definitive" history does not provide the occasion for the comprehensive statement of an underlying thesis, even where it is clearly implicit, as it is in Dubofsky's book. Of what did IWW radicalism consist: of a rather incoherent ideology that its advocates never bothered to clarify; of a vivid rhetoric that never was silenced; of an awesome willingness to go into the streets in the great strikes and free speech fights, to engage in civil disobedience against the illegitimate use of political power, and to accept the beatings and jail terms that invariably were the reward. All this is, of course, well known. What was hitherto not so clear was the extent to which this radicalism served a rather more conservative impulse. Dubofsky makes the following points: the IWW never minimized the importance of im-

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mediate job improvement; never while it was a viable labor movement (that is, until 1919) underestimated the need for effective administration and skilled functionaries; never engaged in sabotage and rarely if ever initiated violence (that was usually the appointed task of the advocates of law and order); did not attempt to impede the war effort in 1917-18; and, despite its rhetoric, sustained a pathetic faith in American fair play and justice (the IWW leaders actually thought they would be acquitted in the 1918 trials). What all this suggests is that the IWW really represented the far end of the pure-and-simple unionism of the AFL: for the industrial dispossessed—the migratory workers, the hard-rock miners and timber hands, the immigrant laborers—not the collective-bargaining tactics of the trade unions, but radical means held out any hope of extracting a fairer shake from the American economic order. The tragic flaw in this strategy was that no one—not even those carrying it out—could keep the essential distinctions clearly in mind. The larger society, of course, took the shadow for the substance, and ultimately crushed the IWW without compunction. It may well be that the IWW pattern, including the sad end, is being relived in contemporary radical movements.

DAVID BRODY
University of California, Davis

The Trial of the Assassin Guiteau *by Charles E. Rosenberg. Chicago, University of Chicago Press, 1968.—xviii, 289 pp. $5.95.*

Professor Rosenberg, a historian, begins with a detailed description of events leading to President James A. Garfield's assassination on July 2, 1881, at Union Station in Washington. The gradual decline of the president, who died September 19, 1881, is described, along with a profile of Charles Julius Guiteau. The assassin, a ne'er-do-well erstwhile lecturer and the son of a religious fanatic, was a hanger-on at Republican headquarters during the 1880 campaign, when the party's factional split was between the Stalwarts and the Halfbreeds.

Guiteau's letter to the public explaining his act is quoted in part: "That the President . . . has been guilty of the basest ingratitude to the Stalwarts admits of no denial. . . . I had no ill-will to the President. This is not murder. It is a political necessity. It will make my friend Arthur President, and save the Republic. . . ."

The political situation of 1881 is well known, but the author presents less familiar material in detailing the background of Guiteau's plea of

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insanity. A dispute on the nature of legal insanity involved Dr. John Gray, Utica Asylum superintendent, strong defender of the M'Naghten Rule, and opponent of the notion of moral insanity, and Dr. Edward C. Spitzka, New York neurologist who described Guiteau's act as the result of congenital insanity. The account of the pretrial activities and the trial itself largely revolves around the bitter professional conflict between these views of the two medical men.

One incident little known about the trial is reminiscent of the Ruby-Oswald confrontation:

> Guiteau was led out to his van. The doors were closed and two guards climbed aboard, one riding in front . . . [the other standing] on the rear steps. Almost immediately the driver started his horses toward the prison. As they drew away from the court, a sorrel horse galloped past and a man mounted shakily on its back drew a pistol and fired at Guiteau through the bars. The shot missed but did leave a bullet hole in the prisoner's new coat. . . .

Thus was Guiteau spared the fate of Lee Harvey Oswald.

Trial testimony of both Dr. Gray and Dr. Spitzka is given in considerable detail, along with summations of defense counsel (including Guiteau's brother-in-law, George Scoville) and the prosecutors, Walter Davidge and J. K. Porter. Remaining proceedings, including the appeal, through the hanging of Guiteau on June 30, 1882, are recounted.

Of the medical men's controversy, the author comments:

> In the broadest perspective . . . the Guiteau trial was an incident not only in the intellectual history of psychiatry, but in the larger development of American society, in the shaping of a modern and institutionalized intellectual life. . . . One can mark the beginning of modern academic medicine and science in the United States from that point in time when American investigators began to care more for [their colleagues' esteem] . . . than they did for the ordinary standards of success in the society that surrounded them. . . .

Rosenberg has written a very detailed, skilled, and perceptive account of the misfit Guiteau and, by implication, suggests how far medical jurisprudence has come in the intervening years. Warren Report critics might well look to this volume for some historical perspective on more recent events.

WILLIAM C. SPRAGENS
Wisconsin State University, Oshkosh

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The Creation of the American Republic, 1776-1787 *by Gordon S. Wood. Chapel Hill, The University of North Carolina Press for the Institute of Early American History and Culture, 1969. —xiv, 653 pp. $15.00.*

Unless they wear the blinders of a particular interpretation, historians who have studied the early national period through the writings of prominent men, as most of them have, can hardly escape the fact that the founding fathers fully believed they were living under mob despotism, or at least the imminent threat of it. The constant theme, reiterated so often as to be a cliché—a casual observation to which agreement was assumed—was that the country had fallen into the anarchy predictable of unchecked democracy and that the licentiousness of the people had to be curbed in order to preserve a republican form of government. If there was a consensus among the nation's leaders, it was this.

What was the elite afraid of? During the Revolution the complaints were about the arrogance of committees and the stupid and arbitrary acts of "popular" legislatures, after the Revolution about paper money and debtor relief legislation. But in wartime somebody had to get out supplies for the army and try to enforce price controls; after the war there was an economic depression, and paper money was the traditional way of coping with it. It would seem that the actual record of events hardly adds up to popular oppression or expropriation. Shays Rebellion, it is true, was an escalation of popular action to higher levels, but it was easily suppressed and got little approval outside western Massachusetts.

Discovering what was behind upper-class fear has been made more difficult by the influence of the consensus interpretation upon recent scholarship. According to the consensus view, which minimizes internal conflict, the Revolution is to be regarded as mere separation from Britain. There was no drive for internal political or social reform. Elite leadership was continuous and unchallenged, except for the Tories. Apart from modifications entailed in shifting to republicanism, the new governments preserved the lineaments of the old and about the same degree of popular participation. It is true that America's official commitment to a liberal version of the sovereignty of the people and the election rather than appointment of all branches of government had long-run democratizing effects. But the Revolution was distinctly not, in Carl Becker's phrase, a struggle over who should rule at home.

Some scholars remain unconverted to this view. Merrill Jensen has always written about the Revolution in terms of democracy, which he

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identifies with majority rule at the state level, countered by an aristocratic principle implicit in national government. Jackson T. Main has documented a democratic trend by showing the number of new men, without wealth or old family connections, who came into the legislatures after independence. In spite of these and other studies, however, it has been possible to think of the Revolution as not involving democracy as an abstract value and of politics as altered in some degree, perhaps, by wider popular participation and a greater need for leaders to solicit popular support, but representing in general a continuum with the colonial past. In this light one could only regard the fear and outrage expressed by gentlemen after the Revolution as an excessive reaction, inspired by marginal diminution of their own status or the rancor of local politics.

Professor Wood's study, which bears on all of these matters, is one of the most important books that has been published on the period in some time. It is a big book, covering nearly everything one might conceive of as relevant to American political philosophy. It is organized by concepts, with philosophy foremost and narrative or description pared to a minimum. Its form and content are derived from that total immersion in the original sources which—at least so we like to think—gives rise to original ideas. Notwithstanding certain limitations, which are the shadow images of its virtues, the book should have a marked impact upon historical scholarship. Since it is already being anthologized, the process should not take long.

Professor Wood makes perfectly clear what it was that alarmed the founding fathers. He unequivocally supports the Progressive view that the Revolution was a democratic movement and the Constitution an elitist counterrevolution. By its propaganda against Great Britain, the colonial elite radicalized the people, creating a democratic ideology that was turned against itself. The upheaval that occurred in Pennsylvania was in some degree repeated in every colony, where the personal rule of elite leaders was challenged by newcomers who exploited the most extreme Whig rhetoric of the Revolution, ridiculing social pretension and distinction. Barriers against popular rule put into early state constitutions were of little avail. The executive was stripped of power; the legislatures dominated all branches of government. Higher property qualifications for the upper house did not ensure that it would contain a set of men distinct from those in the lower house or less responsive to popular will. By the end of the Revolution the idea of elite representation was so unpopular that conservatives defended the upper house only as a procedural check upon the rash or interested action possible in a single house. The Senate merely represented the people in another

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way.

The doctrine of the sovereignty of the people continuously evolved. The idea of granting power to the ruler conditional upon his observing the social contract, first applied to the king, was extended during the Revolution to the elected legislatures. The reserved sovereignty of the people became a force in being, a constant corrective of the legislature, whose decisions were not deemed legitimate if they affronted the majority, or, in the end, even particular interests. Elected officers were straitjacketed by formal instructions from their constituents. The principle of independent judgment was denied; delegates were to voice the interests and opinions of the people. The corollary of this notion was that the people were best represented not by superior but by common men. Legislators were bullied by their constituents. Confrontation politics were the norm. By the mid-1780s, according to the assumptions and standards generally accepted in eighteenth-century political thought, the American state governments were losing the capacity to govern. In establishing the Constitution, the Federalists erected a higher power, presumably beyond reach of democracy in the states. It was a victory of the elite.

This bare sketch, which omits Professor Wood's construction of political philosophy, does scant justice to the scope and complexity of the book. It may suggest a ground of criticism. Professor Wood gives most of his attention and all of his enthusiasm to the progress of radical Whig ideology. Since other writers have said so little about it, he has opened our eyes. The consensus interpretation may never be quite the same. But John Adams and James Wilson were there, too, and clinging to many Americans was a residue of deferential attitudes that the radicals themselves deplored. Conservative thought is not incorporated into the analysis except as a foil. One should also take note of the characterization of the Antifederalists of 1787 as champions of democracy, an idea that runs counter to well grounded studies by Cecelia Kenyon and others. It appears, however, that Professor Wood does not mean the Antifederalists at the Constitutional Convention, who were of the same elite class as the Federalists, but minor politicians, publicists, and unknown newspaper writers in the states, who are the source of much of his material all the way through. A final comment is that he is inclined toward levels of abstraction which, however suggestive, lead at times to verbal contradiction and the danger of inaccurate reasoning. Elite representation as practiced in colonial America, for example, might have something in common with virtual representation in England, but to introduce the English concept with its implications into the discussion of America is to risk admitting a lot of foreign baggage.

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Enough has been said, however, to indicate that this is an outstanding book, one that every scholar will have to read, and since it is well written and interesting in detail, others will read it too.

E. JAMES FERGUSON
Queens College, City University of New York

Twentieth Century Pilgrimage: Walter Lippmann and the Public Philosophy *by Charles Wellborn. Baton Rouge, Louisiana State University Press, 1969.—vii, 200 pp. $6.50.*

Walter Lippmann: Philosopher-Journalist *by Edward L. Schapsmeier and Frederick H. Schapsmeier. Washington, D.C., Public Affairs Press, 1969.—188 pp. $5.00.*

Walter Lippmann celebrated his eightieth birthday, permitted himself a few public political observations on the occasion, and announced he was completing a volume which would serve as a capstone to his ever-developing political philosophy. While these activities signify that any attempt at analyzing Lippmann's ideas is no easy task, for his continues to be a productive and active mind, we are reminded as well that few Americans today fail to recognize his name. For older generations he remains a commentator on American politics; for the present generation, he is but one more voice protesting the Vietnam conflict, the full weight of his words being perhaps only partially appreciated. Lippmann continues to hold that special position as the public conscience, an observer who tells us not only what events are transpiring, but also what effect these happenings are having upon us in our politically democratic order.

Throughout his career, Lippmann has sought the "good society," a civilized society, a society under control. Decision-making, either for matters of foreign or domestic policy, must be in the hands of realistic and rational men who act for the benefit of all, even for those of their fellow-citizens who are uncivilized and selfish. Good leaders act in accordance with what is right and what is right derives from higher law and tradition. Because the supremacy of reason is granted to some, they must perceive the "public philosophy," a higher law than the average man's senses can comprehend. Applying this philosophical perspective to real world situations has been tortuous for Lippmann, resulting in occasional contradictions and momentary paradoxes. Generally, however, he has pledged himself to the comity of states in international relations and to enlightened executives who will secure the domestic gen-

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eral welfare for the citizenry. Clearer details concerning the "public philosophy" have not been developed.

Our interest in Lippmann as political scientists, however, stems from two aspects of his commentary. He was one of the earliest of modern analysts scientifically to assess the problems of a democratic society. Familiar with the psychological approach to politics of Graham Wallas, Lippmann pinpointed the democratic dilemma, saw the citizenry performing according to the dictates of phantoms rather than a public opinion based upon tradition and the basic ideals of American democracy. He was able to identify both the practical and the philosophical problems in American politics, indicate the perils of continuing the same course, and prescribe an alternative which would provide some hope of correction for the disorder. Difficulties have arisen for him, however, in reconciling a prescription for a given situation with the so called "public philosophy" and vice versa. A second level of interest in Lippmann results from his value as a historical anchor. Commentaries on American foreign and domestic policies during the past forty years frequently cite Lippmann, invoking him as a thoughtful contemporary analyzing a given event.

Following Lippmann through his "twentieth century pilgrimage" has been Charles Wellborn, who has put together an excellent distillation of Lippmann's views which give shape and substance to that still to be fully defined "public philosophy." Opening with a brief biographical sketch, the book considers Lippmann's shifting views of the nature of man, the malady of democracy, the meaning and function of law, and the relevance of religion. As the themes are developed, one is impressed with the author's attention to intellectual influences upon Lippmann. Wellborn develops this theme well and with care.

While Wellborn's account limits itself largely to an analysis of Lippmann's philosophy as demonstrated by his writings, the Schapsmeiers' study explores the development of Lippmann's career. Less philosophically grounded and more journalistic, the study traces Lippmann's activities from his days at Harvard to his attacks upon the Great Society and the Johnson administration.

Together, these accounts provide an interesting study in intellectual development, the integration of men and events with the output of ideas. Wellborn assesses Lippmann's critics; the Schapsmeiers devote a chapter to appraisals. Neither account provides a systematic treatment of Lippmann's views on foreign affairs. Though an excellent study was produced seven years ago on this aspect of Lippmann's thought (Anwar Syed, *Walter Lippmann's Philosophy of International Politics*), neither book even mentions it.

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This last comment serves to indicate that the final account of Walter Lippmann's philosophy remains to be written. This is as it should be, for we must recognize that his views are still developing and his capstone has yet to appear. In the interim, both of these volumes, quite excellent in their differing perspectives, will satisfy our needs. And when that final conclusive study is produced, both of these analyses will be cited as erudite and perceptive.

HAROLD C. RELYEA
The American University

Varieties of Police Behavior: The Management of Law and Order in Eight Communities *by James Q. Wilson. Cambridge, Mass., Harvard University Press, 1969.—viii, 309 pp. $6.50.*

More than a third of a century ago, Luther Gulick reminded students of politics and administration that

> much of the actual discretion used in administration is used at the very bottom of the hierarchy, where public servants touch the public. The assessor who walks into the home and sees the furniture and the condition of the house, the policeman who listens to the motorist's story, the health inspector who visits the dairy, the income tax auditor who sees the return and interviews the taxpayer—all these people are compelled to exercise more discretion, and more important discretion from the point of view of the citizenry than many other functionaries much farther up in the organization.

More than twenty years ago, Herbert A. Simon urged more attention to the behavior of "persons at the lowest level of the administrative hierarchy" as an important way of improving insight into the structure and function of an organization.

In spite of the promptings of these eminent authorities, only a few observers of public administrative behavior have turned their energies to this line of investigation. After all this time, we have at most a handful of studies of the way the decisions and actions of the people who actually do the physical work of organizations are influenced within and by the organizations to which they belong. It is gratifying to have this small body of literature substantially enriched by Professor Wilson's stimulating book on police behavior.

Drawing largely on his intensive studies of the police in eight communities "picked pretty much at random" (Albany, Amsterdam, Brighton, Nassau, Newburgh, and Syracuse in New York; Highland Park, Illinois; Oakland, California), he focuses his analysis on the patrolman. His evidence, which is mainly anecdotal and illustrative but impressive-

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ly rich in detail, documents the assertion that the policeman on patrol does indeed exercise significant discretion. The question to which Wilson addresses himself is: what determines how that discretion is exercised?

His answer is that the patrolman is told what he must *not* do, but there is little to guide him in what he *ought* to do. The rulebooks do not help him in specific situations because each situation is unique; the rules are either ambiguous or irrelevant in many cases. There are no professional canons or operational standards of performance to instruct him. In the end, therefore, each department develops its own style of action, depending on the character of the community in which it functions. Some adopt a "watchman style," ignoring many violations, but cracking down on offenses considered serious; such departments elect "to use the law more as a means of maintaining order than regulating conduct." Some choose a "legalistic style," relying extensively on their formal powers of arrest and complaint rather than on conciliation or on "delaying arrest in hopes that the situation will take care of itself." Some choose a "service style," seldom tolerating violations, but invoking formal authority less frequently than do legalistic departments; they seek fuller compliance than do the watchman departments, employing cooperation rather than coercion as the means. The individual policeman thus learns how to use his discretion by a process of socialization to the prevailing style of his department.

Some who seek to improve police performance tend to try to bureaucratize the function, advocating more numerous and more specific rules to reduce discretion. Others think "professionalizing" the force, and then allowing its members even greater discretion to maintain good community relations, is the promising course. Wilson is skeptical of both, arguing that the policeman is neither a bureaucrat nor a professional, but a member of a craft, and neither "reform" is likely to win police support or, consequently, to work. He also has doubts about some proposals for neighborhood control of the police: distinguishing between dispersal of police authority, which has high costs and few benefits, and decentralization of police functions, which is another matter entirely, he obviously believes some commentators have confused the two. Decentralization under a strong central command is what he favors. He also suggests it may be a mistake to expect municipal police to handle riots as part of regular police operations and talks briefly but approvingly of specially trained disorder-control forces. But Wilson is a realist, and sees no prompt and dramatic solutions for the tensions between the police and the community—particularly between the police in relation to the poor and the blacks—because the community itself is ambivalent about what it wants of its officers. There is no blue-

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print for utopia here, only the information and insights to expose the simplemindedness of most of the extreme proposals in this field.

Wilson paints on a large canvas, using a broad brush and sweeping strokes. The effect is to provide what we need very much—a sense of the dimensions and the contours of the problem we face. But he whets the appetite for aspects of it that his method does not illuminate. We see the police *responding* to pressure, for example, never molding opinion or *bringing* pressure to bear. We are shown their dilemmas, not their resources and their allies. We learn a great deal about their frustrations, little about their legislative and collective-bargaining victories. Most agencies come to terms with organized interest groups; as protrayed in the analytical sections of this book, the police seem to deal with individual citizens or the community at large. In short, the police themselves emerge in reasonably sharp focus, but their linkages with their environment are somewhat blurred.

These are limits rather than limitations of the book; no author can do everything, and Wilson does well and interestingly the task he set for himself, a study emphasizing heavily the internal dynamics of police organizations. In achieving this goal, he has not only advanced our knowledge of these public agencies and of public agencies generally; he has also shown us how much remains to be done before we have a full picture of that singular set of public officers and employees who in so many ways symbolize the authority and the will of the state and the community.

HERBERT KAUFMAN
The Brookings Institution

Voting for President: The Electoral College and the American Political System *by Wallace S. Sayre and Judith H. Parris. Washington, D.C., The Brookings Institution, 1970.—xii, 169 pp. $5.95.*

The continuing debate on reform of the electoral college system, as the authors of this monograph effectively demonstrate, has been characterized by an astonishing disregard of the likely political consequences of the proposed changes. Not only proponents but also most critics of suggested reforms have failed to examine these proposals in a way that assesses their likely effects upon the complex fabric of the American political system and, in particular, upon the institution of the presidency. This deficiency need exist no longer. The availability of this monograph

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should compel participants in future debates to confront the question of political consequences, whether or not they accept the conclusions arrived at by the authors.

In estimating political impacts, Sayre and Parris make no claims on certainty. In fact, they explicitly argue that since only the existing system is a tested fact and the effects of proposed alternatives are thus in varying degrees speculative, the burden of proof lies with those who advocate change. The authors' speculations nevertheless are firmly based on the data and are persuasive. They properly insist that the issue in debate is not, despite assertions to the contrary, whether the president should be elected by popular vote but how the popular vote should be aggregated. To the various plans for aggregation, including the present arrangement, they put a series of diagnostic questions, each of them bearing upon the crucial matter of what kind of presidential constituency is most desirable: will it produce a prompt result, without frequent resort to contingency procedures? will it produce presidents with a mandate to govern that is adequate to the requirements of the system? will it confer legitimacy upon the president and the presidency in sufficient measure to maximize acceptance of the result by citizens and leaders? will it maintain other established patterns of the American political system—the separation of powers, an evolving federalism, the national two-party system, and political stability in critical transitions of power and policy? They make clear their own value preferences: a national two-party system, a strong presidency, a balanced federalism, and effective access to the presidency for metropolitan areas and their residents, especially the newly emerging minority groups.

By these standards each of the proposed reforms is found wanting, most of them seriously, and the existing system, despite its defects, superior. Each of the three major proposals—the direct-vote plan, the district plan, and the proportional plan—by abolishing the state general-ticket system would be expected to undermine the two-party system, to weaken the political position of the metropolitan areas, and to develop a fragile presidency. In addition, the district plan would promote an extreme localism, and the direct-vote and proportional plans would encourage nationwide minority factionalism. The proportional plan would increase the possibility of prolonged indecision and would be seriously divisive. The direct-vote plan would increase the likelihood of a minority president and would involve dangerous dependence upon contingency run-off elections that would produce presidents with weak and disputed mandates.

The authors consequently recommend that the electoral vote system and the general-ticket system be retained. They suggest that most ob-

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jections to this existing system can be met through action by the national party organizations, the members of Congress, and the state legislatures without resort to constitutional amendment. If an amendment is deemed necessary, they support the automatic plan, which would eliminate the office of presidential elector but retain the requirement of an electoral vote majority and the general-ticket system. In the unlikely contingency that no candidate received an electoral vote majority, they would favor a provision for the president either to be chosen from the top two (rather than three) candidates by majority vote in the House or a joint session of Congress or to be designated from the ticket winning a plurality of the popular vote.

It is implicitly a tribute to the strength and stability of the American political system that the debates on reform of the electoral college have largely ignored the probable effects of proposed alternatives upon the system. Although this tacit confidence may be reassuring, it is a dangerous reliance in contemplating changes affecting the keystone of the system, the presidency.

DAVID B. TRUMAN
Mount Holyoke College

Vietnam and China, *1938-1954 by King C. Chen. Princeton, Princeton University Press, 1969.—xv, 436 pp. $12.50.*

Geneva *1954*: The Settlement of the Indochinese War *by Robert F. Randle. Princeton, Princeton University Press, 1969.—xviii, 639 pp. $17.50.*

These two books, presenting complementary historical analyses, are useful additions to the ever-growing literature on Vietnam. They fill important gaps with meticulous research.

The major contribution of Dr. Chen's book is his treatment of the period from 1938 to 1945, when Ho Chi Minh was in China. Chen provides the most careful chronology and discussion to date of Ho's activities, the role and motives of the Nationalist general, Chang Fa-k'uei, and the part played by the American O.S.S. in the Viet Minh's coming to power. Making use of interviews, the author relates how Chang and Ho hoped to and did use one another to further their respective strategic and political ambitions in Vietnam. And Chen also draws our attention, as have other writers, to the opportunities the United States lost to cultivate friendly relations with the Democratic Republic of Vietnam following the end of the Japanese occupation.

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Chen's book is also informative on the policies of the Viet Minh in the period of the Chinese Nationalist occupation (1945-46) and on the manner in which the French authorities handled the delicate problem of some 30,000 Kuomintang soldiers who fled into northern Vietnam in 1949 in the wake of the Communist takeover of China.

The book is weakest on the Geneva Conference and its implications for Sino-Vietnamese relations. Chen's discussion of Chinese motives and objectives at the conference is limited; too much attention is devoted to well known facts (the agreements reached and a chronology of events) or to relatively unimportant data (Communist propaganda at the conference) and too little to analysis. His concluding chapter, moreover, is disappointing in its failure to make sufficient use of the opportunity to compare Chinese Nationalist and Communist behavior on the question of Chinese "influence" in Vietnamese internal and external policies. The author shows clearly *how* Chinese influence was brought to bear, but without distinguishing clearly among influence, leverage, and control. He stresses that 1950-54 was the high watermark of Vietnamese Communist emulation of Chinese programs and ideology, for instance; but was this development occasioned by conviction or a "pro-Chinese" faction's predominance, as he insists, or was it primarily the result of the Viet Minh leadership's practical recognition of its weaknesses and of China's willingness and capacity to help? The answer is important in evaluating the recent history of Peking-Hanoi relations.

Professor Randle's work combines historical, political, and legal analysis. This approach is both an asset and a liability. On the positive side, *Geneva 1954* is the most comprehensive treatment available of the events preceding the conference, the path to agreement, and the implications of the "accords" for the political future of South Vietnam. The most valuable and original part of the book assesses the Final Declaration and the agreement on the cessation of hostilities by drawing upon international law as well as the motives and actions of the participating parties. Some of Randle's conclusions should stimulate controversy, but his methodology is sound and his interpretations plausible and balanced.

Elsewhere, however, Randle tends to be tedious and verbose in his concern for completeness. He offers hundreds of pages of detailed discourse, too often in the manner of a legal brief and without adding to what has been said already in several other books. Not that his prodigious effort does not produce interesting evaluations—such as that Secretary of State Dulles's diplomacy during and after the fall of Dien Bien Phu was cautious, restrained, and perceptive, a conclusion that will probably strike many readers (including this one) as novel and contestable. But, overall, the effort to be exhaustive may prove exhausting for

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many readers, not because of the length per se, but because the significant analytical portions in the last third of the book may be obscured by introductory and background material.

MELVIN GURTOV
University of California, Riverside

Politics and Social Conflict in South India: The Non-Brahman Movement and Tamil Separatism, 1916-1929 *by Eugene F. Irschick. Berkeley, University of California Press, 1969.—414 pp. $9.75.*

As the nationalist phase of modern Indian history fades into the past, that period is beginning to receive the sophisticated analysis which its richness and variety merit. We have learned that there was not a single, mass, nonviolent movement led by Mahatma Gandhi, but rather a sprawling complex of recently mobilized publics, complicated social, cultural, and regional cleavages, and often warring elites and personalities which was never really united by Gandhi or the Congress leadership. The partition of 1947 made the Hindu-Muslim split starkly real to outside observers and to Indian nationalists who had long believed it a mirage conjured up by Mr. Jinnah and a handful of spiteful Muslims. Dr. Irschick's book and a number of others published in recent years give us a fuller sense than we have had of the interplay of regionalism and nationalism and of the conflicting social, cultural, economic, and political pulls which many Indians experienced during the decades preceding 1947.

Dr. Irschick, who himself grew up in South India, deals with a number of interrelated developments in the Madras Presidency during the second and third decades of this century that have had and have now significant implications for contemporary India. He describes the social, cultural, and political peculiarities of South India in a clear and unpretentious style, and he draws upon a wide range of materials in English and vernacular languages. Analyzing developments in a usually neglected region of India, he pays particular attention to the social cleavages between Brahmans, non-Brahman-caste Hindus, and untouchables; to the assertions of the importance of Tamil culture as opposed to the dominant Sanskritic culture of Indian Hindus; and to the subregional clashes between Telugus and Tamils. The central focus is upon the non-Brahman leaders who developed a movement cultural, social, and political by turns, but the author keeps his eye as well on other relevant events in India and Great Britain.

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Some of the non-Brahman leaders in Madras formed the Justice party, which reached its zenith and began its rapid descent within the 1920s. This party was the most political expression of the non-Brahman movement before independence and, as Dr. Irschick shows, brought about its own downfall by narrowing its aims and social base once it had become successful in the Madras legislative assembly under the Montagu-Chelmsford reforms in the early 1920s. Its regional and restricted outlook and its concern with limited goals for non-Brahman caste Hindus soon drove the untouchables and some of its own membership out in search of other political possibilities. But despite the decline of the Justice party, the non-Brahman movement continued and gave birth to the Self-Respect group in the middle 1920s, the D.K. in the 1940s, and the D.M.K., the present ruling party in Madras state. The Justice party helped to bring some short- and long-range gains for non-Brahmans and also began the mobilization of the untouchables which subsequent organizations continued. Although he treats the Justice party in most detail, Dr. Irschick also presents an interesting account of the Congress in Madras during these years and explains how the Brahman versus non-Brahman conflict affected the fortunes of the major nationalist organization in South India.

The author writes in a detached and unemotional way though it does seem that he disapproves of communal representation and the narrow aims of the Justice party. These implicit judgments could have been elaborated upon and their consequences explained; the author also might have organized his material so that Annie Besant was not displayed so prominently in what was, after all, an Indian movement; and he might have considered the Congress organization in a more analytical fashion. But these are small quibbles with a fine, lucid, and important book on Indian history.

LEONARD A. GORDON
Columbia University

The Life and Death of Mahatma Gandhi *by Robert Payne. New York, E. P. Dutton and Co., 1969.—703 pp. $12.95.*

Gandhi's Truth: On the Origins of Militant Nonviolence *by Erik H. Erikson. New York, W. W. Norton and Co., 1969.—474 pp. $10.00.*

The torrent of Gandhi literature continues, two decades after the Mahatma's death and into the centenary of his birth. The 1969 vintage

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was unusually good, with an excellent biography by Robert Payne and a notable analysis by Erik Erikson.

Robert Payne has something of the reputation of being a writing machine, but the machine operator at least for this volume gave way to the artist and the result is a first-rate biography. Good biographies of Gandhi have been published almost each year since 1948, and Payne's is among the best, except perhaps for the late Louis Fischer's early *The Life of Mahatma Gandhi.*

Payne writes an unusually perceptive introduction. Stripping away the legends around Gandhi, he affirms that Gandhi "wore many public masks and many private ones, and sometimes, like all men, he mislaid the masks and showed himself naked." Payne, like other biographers before him, is fascinated with the multiple roles of Gandhi—"a virtuoso performer . . . the author of the play, the stage manager, and most of the players." Payne continues the faults of most biographers by devoting more space proportionately to the early years of Gandhi's life—up to his return to India in 1914—than to the 34 years afterward. But even Gandhi himself wrote more about this earlier period.

Payne's book is exceptional in devoting three chapters to the conspiracy on the assassination of Gandhi. Payne reaches the astonishing conclusion that "there were many people in high places who acted as though they had no business interfering with a conspiracy which must be permitted to take its course." He adds: "By negligence, by indifference, by deliberate desire on the part of many faceless people, the assassination had been accomplished. It was a new kind of murder—the permissive assassination, and there may be many more in the future." Of course, by the time Payne had written these words two Kennedys and Martin Luther King had already died.

Erik H. Erikson, the Harvard professor best known to the public for his *Young Man Luther,* has now made an epic study of the middle-aged Mahatma. If Gandhi had multiple roles, so does Erikson. His book is a kind of *Moby Dick,* with Gandhi taking the place of the whale! This book is a travelogue, a clinical case study, a psychoanalytic commentary on Gandhi's autobiography, and some original contributions to the dynamics of Gandhian nonviolent action. The book includes a long critique which, were Gandhi alive, Erikson would have relished confronting him with.

Shortly upon Gandhi's return to India from his formative years in South Africa, he became involved in a mill workers' strike in Ahmedabad. Erikson calls this strike the "Event" in Gandhi's life history—not his identity crisis (which occurred 25 years earlier in South Africa), but still a crisis of considerable personal and political proportions. Erikson

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analyzes the strike for many pages, while Payne—like most biographers—gives it scarcely a chapter.

Erikson sees a strange reversal in Gandhi of traditionally held (but not universally valid) roles of East and West. Gandhi was a model of activism, while Western, post-Freudian thought often provided for new techniques of introspection. Erikson naturally compares Gandhi and Freud. Both wrote their great introspective works in middle age and on the threshold of lonely greatness. Both attempted to enlarge human awareness, with one complementing the other. Both offered "candid descriptions of small events." Both knew that human insight begins with oneself—Gandhi probing his motivations and Freud his dreams. The psychoanalytic method is a counterpart of the Satyagraha method, since both confront the inner enemy nonviolently. Erikson calls this a correspondence in method and a convergence in human values "which may well be of historical, if not evolutionary, significance."

Putting Gandhi on the biographical couch, Erikson probes his strange sexual theories and his "truly dictatorial combination of materialism and paternalism." Erickson early gives Gandhi, if not always his theories, a complete and effusive endorsement: "I can only view with awe a man who (making himself more transparent than any of the saviors and saints of the mythologized past) improvised every item in the inventory of saintliness—nakedness, poverty, silence, chastity, and charity—without being baptized or ordained in any traditional investiture; and who attempted to apply the power of that position in every waking minute to the Here and the Now as lived by the masses of men."

The last word has not been written about Gandhi, but the two large volumes by Payne and Erikson are complementary additions to the literature which is notable for its bulk and repetition more than for new insights.

HOMER A. JACK
Boston University

The Ordeal of Nationhood: A Social Study of India Since Independence *by Krishan Bhatia. New York, Atheneum, 1971.—390 pp. $10.00.*

This broad review of India's development as an independent nation is both sufficiently lucid in style and clear in organization to be useful to readers unfamiliar with the subject and, at the same time, knowledgeable enough to provide students of India with some new insights.

Mr. Krishan Bhatia, who is one of India's most respected journalists, has had a unique opportunity to watch the development of the Indian

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economy and Indian political system as a reporter and friend of many key figures in the Indian government.

Out of necessity, as a journalist, he has had to deal for some 25 years with seemingly contradictory facts and conditions in close juxtaposition; this plus the perspective gained by his years outside India has enabled Mr. Bhatia to strike a fair balance between India's achievements since Independence and the difficulties which are still to be overcome.

On the positive side of the ledger is India's courageous adherence to free institutions in spite of the staggering economic, political, and social problems with which it has been faced since Independence. In its 25 years of independence, India has somehow managed to absorb a series of blows that would have destroyed a less resilient society. There have been five general elections and only three prime ministers—Jawaharlal Nehru, his daughter Indira Gandhi, and Lal Bahadur Shastri, each brilliantly effective in his own way. India's achievement in maintaining its democratic institutions under heavy pressures is unique; it is now one of the few remaining democracies in Asia and Africa.

Since Independence, India has also made impressive progress in economic development. Electric power capacity has sharply increased; the railroads have been modernized and expanded with much new equipment built in India. Exports have steadily increased with industrial goods, machine tools, bulldozers, and the like gradually replacing India's traditional export dependence upon jute and tea. India's success in adapting the new hybrid seeds to Indian soil and conditions, in developing a major fertilizer industry, and in more than doubling land under irrigation has led to a sharp increase in wheat production and suggests the probability that by 1972 or 1973 it will be self-sufficient in food grains. Malaria, which used to strike more than one hundred million people each year, is no longer a major problem. India's defense capacity has been vastly strengthened since the brief fighting between China and India in 1962. At the same time the military has continued to remain aloof from politics.

On the negative side of the ledger, Mr. Bhatia points out certain dangers which should not be minimized. While the Green Revolution has created a remarkable sense of change in the Indian countryside and instilled new confidence in the Indian people, increasing population remains a serious threat. The Indian population-control program is by all odds the biggest in the world today. But Indian leaders are acutely aware of the fact that the steady increase in food production may be balanced off by continued increase in population. Land reform designed to provide a more solid political base in the rural areas has lagged. Moreover, the Green Revolution has thus far been confined largely

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to wheat; rice production has increased at a much slower pace.

Caste differences have gradually been eased with the help of legislation not unlike that with which we have dealt with our own racial problems. But conflicts between India's Hindu majority and the sixty million Muslims who chose to live in India continue to be worrisome. Although the dominant Congress party under Mrs. Gandhi's bold and skillful leadership has been miraculously rejuvenated, a two-party system thus far has not been attained. Opposition to Mrs. Gandhi's government has been provided by a conglomerate of parties ranging from the extreme Right to the extreme Left. Progress in education has been below expectations. The percentage of illiterates is still high, and much of India's educational system remains stuck in the mold of the old British tradition.

These and other complex questions Mr. Bhatia covers with thoroughness, sensitivity, and objectivity. Indeed, if I had any one criticism to offer it would be his tendency to underplay India's achievements since the British left in 1947. Because of the vastness of India's remaining problems, many of its most impressive achievements are overlooked or minimized.

The conflict in East Pakistan which occurred after this book was completed has placed an additional strain on the Indian people and their government, the extent of which is not yet clear. In order to feed and care for the nearly eight million refugees from East Pakistan, India has had to dig deeply into its own food reserves and abandon some crucially important development programs.

Bhatia also digs beneath the often sad story of American-Indian relations to describe the misunderstanding between these two great democratic nations. India has been deeply critical of our military action in Indochina, of Washington's failure to recognize India's efforts to maintain the kind of open democratic society which we have always claimed to favor, and of the State Department's insistence on assigning India and its much smaller totalitarian neighbor, Pakistan, parity in the determination of American foreign policy in South Asia. Americans, for their part, have been put off by India's tendency to overcompensate for its historic and linguistic ties with the West by criticizing the West for actions which it might overlook if committed by the Soviet Union. Given the sorry record of America's present relations with India, Krishan Bhatia's readable and concise book should help build an understanding of India among the American people at this time, an understanding urgently needed and greatly to be welcomed.

CHESTER BOWLES
Essex, Connecticut

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A View from New Delhi: Selected Speeches and Writings *by Chester Bowles. New Haven, Yale University Press, 1969.—268 pp. $7.50.*

Uncertain India: A Political Profile of Two Decades of Freedom *by Pran Chopra. Cambridge, Mass., The MIT Press, 1968.—vii, 403 pp. $7.50.*

Bowles's book is a collection of speeches and articles turned out by the U.S. ambassador to India from 1963 to 1969. The author had two objectives in mind: first, to give the Indian people and their government a better understanding of America's objectives in world affairs and what the Americans learned by costly trial and error about economic development; second, to give the younger generation of Indians greater confidence in their country than they get from their usual news media. It is not at all an easy task for any diplomat to be accepted as something more than a mere propagandist for his country. But Bowles's great passion for useful facts and feeling for their interesting presentation give his writings and speeches a special appeal. His chapters on the prospects for Indian agriculture and India's family planning are replete with useful information which is not available in current Indian literature. He would have certainly succeeded more in his objectives if, instead of reprinting a selection of speeches and writings, he would have collated his ideas and facts with a more meaningful thread of continuity and avoided some obvious repetitions. It is difficult to find a systematic set of ideas in writings and speeches which were inspired by diverse varieties of occasions and events. But many of these reveal clearly the insightful understanding of the economic and political problems of India and other developing countries. In a penetrating analysis of the successful economic development in Japan, South Korea, and Taiwan, he found that the basic ingredient of rapid economic growth was the widespread ownership of rural land. He predicted that "the ultimate answer to the explosive question 'Who owns the land?' will shape the economic and political future of Asia, Africa and Latin America." There is no doubt that at least in a few states of India—for example, West Bengal—the current unrest among the peasants fully justifies the author's prediction. The book is worth reading for those interested in the current economic problems of India and in Indo-American relations.

An interesting aspect of Chopra's *Uncertain India* is that he is not quite uncertain about India's political future. Contrary to the obvious conclusions one may make from the recent newspaper headlines about disruptions, violence, instability of the state governments, and splits in

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the political parties of India, Chopra discerns healthy trends in the current functioning of Indian democracy. An analysis of the voting behavior in the four general elections, particularly the last one, leaves little reason for him to doubt that "the Indian voter votes with discrimination and without fear, that his interest in the democratic process is rising, not falling, and that the influence of the local factor upon him, especially of caste, though still strong, is also falling." I am, however, doubtful whether a political scientist or an anthropologist using his modern tools of investigation would agree with this conviction. But the importance of the present book lies not in the predictions or convictions of its author. He has fully utilized his long and rich experience in Indian journalism (editor of the distinguished daily *Statesman* for some years) to describe the evolution of the Indian political system. The description seems to the present reviewer as unbiased as any journalistic description could be, but it suffers from such excessive details at many points that the reader is likely to lose the main thread of significant events which shaped India's political system as it stands today. One of the objectives of the book is to make India's recent past and the present comprehensible to the lay reader, especially overseas. The book is expected to fulfill this objective to a great extent and also remain as a faithful chronicle of selected events in the recent past.

MONI NAG
Columbia University

Japan's First General Election, 1890 *by R. H. P. Mason. Cambridge, Cambridge University Press, 1969.—viii, 254 pp. $7.50.*

This book is an informative addition to the growing shelf of literature on prewar Japanese party politics. Based mainly on research in newspapers of the time (primarily the *Tōkyō nichi shimbun*), it provides a concise description of the technicalities of the balloting process, a review of the major political parties and their platforms, a survey of campaign techniques, and a cursory statistical sketch of the results of the 1890 election. The author's wit, humanity, and sense of style, which shine through particularly in his vignettes of campaigns conducted by "self-nominating" candidates, give life to what otherwise might have been drab description. How could one not like a book that describes an unsuccessful candidate, who threw lavish banquets for his constituents, as going down to defeat "amid the clatter of plates and glasses."

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The main contribution of the book is the vivid way it illustrates the importance of local political involvement in the early years of Japan's parliamentary system. As Mr. Mason points out, this was very much in contrast with other non-Western countries where the reception of representative institutions "made some headway among the intelligentsia of the cities but failed in rural areas" (p. 206). The Diet was not simply a structure imposed from above on an ignorant or passive population, but was sustained from the start by the interest of the provincial men of substance ("village gentry") who made up the bulk of the new electorate. It was from this class that most of the successful candidates were drawn, for electoral victory often depended more on local reputation and family status than on anything else. It was also to this class that the political parties, at least those "left of center," appealed with their populistic attacks on the centralizing, procommercial, and antirural bureaucratic biases of the tax-gobbling government in Tokyo. After reading the book, it is easy to understand how a publicist of the early 1890s like Tokutomi Sōhō could attach such importance to the role of "provincial gentlemen" (*inaka no shinshi*) in the political future of Japan.

One can raise questions, however, about the author's evaluation of this political localism. He suggests that it essentially continued the *han* and village localism of the Edo period and that it represented the surfacing of an individualism—by which he seems to mean a willingness to pursue one's own self-interests with little regard for group harmony—that likewise had roots in the pre-Restoration period. This kind of generalization is useful to the extent that it suggests a politicized countryside did not spring into being solely as a result of post-Restoration reform. On the other hand, might it not be more to the point that the activities of the men who participated in the 1890 election were a quantum leap away from the political activity their grandfathers could have indulged in? They were *soliciting* positions of political responsibility from their fellows, not simply *inheriting* traditional offices; they were openly expressing opposition to government without fear of physical reprisal; they were linked (albeit loosely) through membership in national political organizations; and so forth. Surely these facts are of some significance and require more comment.

This reviewer wishes that the author had lowered his sights interpreting his research results and that he might have looked less far backward or a little farther forward when fitting them into historical perspective. It might have been useful for him to speculate, for example, on how the prefectural assembly politics of the 1880s paved the way for the 1890 election. The recent work of Masumi Junnosuke (not men-

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tioned in the bibliography) has shed much light on this question. Similarly, comparison with later elections, (say) the first universal manhood suffrage election of 1928, might also have been instructive in showing how patterns of electoral behavior evolved after the 1890s.

In short, despite the meticulousness with which the author did his research and the felicity with which he reports it, the book suffers from a failure to extract the full measure of the election's historical significance.

PETER DUUS
Harvard University

Sōka Gakkai, Builders of the Third Civilization: American and Japanese Members *by James Allen Dator. Seattle, University of Washington Press, 1969.—xiii, 171 pp. $7.95.*

The growth of the Sōka Gakkai as a political force in Japan over the last decade has produced a considerable periodical literature in recent years. For this reason Professor Dator's excellent monograph is especially welcome, since it provides both specialist and nonspecialist with an easily accessible and well written analysis.

Starting from the hypothesis that the Sōka Gakkai is essentially a value-oriented movement, Professor Dator utilizes data from several Japanese surveys and from one of his own devising to test the validity of this assumption. By reference to the voluminous literature on sect and cult demographic and personality characteristics, the author developed a survey which was applied to a random sample of Tokyo residents, a technique which allowed for comparative analysis between Sōka Gakkai members and nonmembers. The presentation and analysis of these data provide a generally excellent profile of the Japanese and a rather more impressionistic one of the American membership.

Professor Dator deduces that, as with many other sect-cult movements, membership is drawn from the "relatively deprived," a group with neither the knowledge or economic power to deal meaningfully with their grievances. The author thus establishes the existence of one of the basic elements of structural conduciveness for the emergence of such movements. At the same time, the author also recognizes that this element was a necessary but not sufficient cause for its emergence. He suggests that to this conduciveness were added the strains of the impact of urbanization generally and specifically the trauma of defeat on normative behavior. The author also concludes, perceptively, that a major source of the Sōka Gakkai's appeal is its totalistic and simplis-

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tic explanation and solution of individual and social problems. At this point, however, Professor Dator fails to distinguish carefully between success in attracting members and success in maintaining organizational growth and continuity. Only slight attention is paid to the factors which have apparently produced high levels of commitment and its concomitant, organizational strength. Thus, for example, the function of the bureaucratic organization of the structure with its emphasis on "career" and promotion in providing not only psychic rewards to the participant but also a continuing flow of uniformly trained and socialized personnel necessary for organizational continuity and stability is given scant mention.

These are not serious shortcomings, however, in an otherwise logically developed analysis. Yet, at the very close of the study Professor Dator, by pointing out how nonreligious the Japanese are (pp. 140-41), inadvertently raises the most significant question concerning the Sōka Gakkai phenomenon and then promptly ignores it. As J. L. Gillin observed in a now classic analysis of religious sects, "religious sects will arise only when religion is the dominant interest" ("A Contribution to the Sociology of Sects," *American Journal of Sociology*, XVI [1910-11], 246). Value-oriented movements tend to emerge in societies where values and norms are not well differentiated. How then can we explain the postwar "rush hour of the gods" in Japan, which saw the emergence of a great number of sects and cults of which the Sōka Gakkai was one? If Japan is not a religious society, why did "relative deprivation" take the form of religious movements? Can it be that despite Japan's political and economic complexity the norm-value structure remains fused in a manner which approximates systems of lesser complexity and, if so, how can this paradox be explained? It is disappointing that a monograph of such quality should have avoided the question at which, by its own logic, it was directly aimed.

BERNARD S. SILBERMAN
Duke University

How the Conservatives Rule Japan *by Nathaniel B. Thayer. Princeton, Princeton University Press, 1969.—xiv, 349 pp. $10.00.*

All too often we are reminded how ignorant we remain about the politics of the third largest economy in the world, Japan. Basic descriptive knowledge of the political decision-making processes in the United States and major European countries is available in textbooks, but for

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Japan the need for a comprehensive account of the political processes attached to the formally constituted governmental units has only now been partially filled by N. Thayer's *How the Conservatives Rule Japan*. This is a book that should be read by anyone interested in contemporary Japanese politics.

The author has written a how book, descriptive rather than explanatory. He focuses on the decision-making process of the dominant Liberal Democratic party (LDP), how the LDP Diet members are elected, and the relationship between the party and the economic community. There is an analysis of the relations between the party and the prefecture through a case study of the political organization of Nakasone Yasuhiro, a prominent LDP politician. In addition, the author discusses cabinet formation and the formulation of public policy. The work suggests other largely untouched problem areas, such as the impact of public policies on their intended subjects.

The author's method of analysis was well suited to his intent and opportunity. Imaginatively building on personal relationships developed during his tenure in the former Bureau of Far Eastern Affairs in the State Department and as press attache of the American embassy in Tokyo, he conducted in-depth interviews with relevant party leaders. In view of the known difficulties of gaining access to Japanese politicians, the study is all the more valuable. Where relevant, the author summarizes basic social-economic and political characteristics of the Diet members, often for the first time in English.

Very wisely, the author for the most part eschews value judgments concerning the goals and orientations of Japanese political leaders. This is best seen in his discussion of factionalism within the Liberal Democratic party, the best summary of the subject I have yet seen. Here he is content to describe the functions of the factional system rather than to attribute negative results to it based on his own value perspective. Cautiously, he does present evidence that the political party as opposed to the faction may be emerging as the more important base of activity.

The analysis is weakest when the author makes his projections. He correctly points out the greatest difficulty facing the LDP, the absence of effective transmission belts between the local level and the party leadership. But it hardly advances our understanding of the problem to conclude that "The future of the party lies with the people" (p. 318). A statement like this is made because description is not carried out within a theoretical framework. For example, may we subsume LDP behavior under an explanation based on the level of political party institutionalization? Or between the level of political party institutionalization, political participation, and the rate of economic development?

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Structural categories of analysis must be employed if we wish to explain the kind of behavior described so usefully by N. Thayer.

ROGER W. BENJAMIN
University of Minnesota

Chief, Council and Commissioner: Some Problems of Government in Rhodesia *by J. F. Holleman. Assen, The Netherlands, Royal Van Gorcum Ltd. on behalf of the Afrika-Studiecentrum, 1969.—xix, 391 pp. Hf 1.29.*

Rhodesia: A Study of the Deterioration of a White Society *by Frank Clements. New York, Frederick A. Praeger, 1969.—286 pp. $6.50.*

Both these studies of government and society in Rhodesia have been written out of a sense of personal involvement and professional disillusionment with Rhodesian politics of the past two decades. Clements, a journalist, held office as mayor of Salisbury during the Whitehead regime; Holleman served as field anthropologist and urban administrative officer before taking up academic life at the Universities of Natal and Leiden. That their books are not wholly academic both writers make explicit, but whereas one is a scholarly study of native administration derived from a report of a commission of inquiry that proved a landmark in the study of relations between administrator and chief in Central Africa, the other contains little of substance that could not have been gleaned from the pages of the *Rhodesian Herald* or *Daily News*.

Clements's book contains eighteen chapters bearing such titles as "Two cheers for Democracy," "... But some are More White than Others," and "The Nigger in the Woodpile"; its subject matter, at best, draws heavily on Hanna, Gann, Leys, Mason, and Ranger and, at worst, on journalistic impressions of the Rhodesian scene. Clements refers to his experience as mayor of Salisbury only to support a contention that public opinion was swinging against both African parties (ZANU and PCC) prior to their being banned in 1964. While accepting Leys's demonstration that political life in Rhodesia in 1959 was indeed founded upon a divorce of European politics from African political activity, it seems wholly unreal in 1970 to read a study of Rhodesian politics in which Africans are indeed, as Clements observes, "part of the landscape" rather than part of the community (p. 57).

The explicit subjectivity at times of *Chief, Council and Commissioner*

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is, on the other hand, part of its strength. It is valuable no less for its extended case analysis (only yet too rare in African political studies) than for future comparative work in development administration. Its method of analysis is aimed at "the interaction between (opposing) group interests and at the conflict of value systems which, though derived from different cultures, were forced together into a single socio-political context" (p. 167), a characteristic problem area in the study of cultural pluralism.

At the heart of the book lies an analysis of the term of office from 1937-59 of Chief Munhuwepayi of the Shona Nhowe tribe, a nontraditional, progressive chief who was obliged to become increasingly authoritarian at a time when chiefs were losing their hold upon the people. As a preliminary, Holleman sets out the structure of the Native Administration in Rhodesia prior to its expansion and increasing strain under the implementation of the African Land Husbandry Act of 1951 and the African Councils Act of 1957, both of which were to underline political conflict in Munhuwepayi's Mangwende tribal area. The radical activities of the chief in the Kraal-heads' Association and the local council, both European-introduced institutions of representative government, led to his being called before a commission of inquiry in 1959 and his subsequent deposition. The client chief, walking a tight rope between his people and his superiors, is a now familiar figure in African studies. What is of special interest in the Rhodesian case is that, appearing a decade later than his counterparts elsewhere on the continent, he has an intriguing role to play in both nationalist and white secessionist politics. This is brought out clearly in the concluding section of the book based largely on Holleman's research in Rhodesia during short visits between 1964 and 1966. To many, his concluding chapter on the "rediscovery of chiefs" with its inevitably inconclusive ending will prove one of the most interesting in the book.

Besides its historical interest, Holleman's book touches upon many issues of current concern in the study of local and administrative politics in Africa as well as some of the wider theoretical issues of social anthropology and comparative politics. He is concerned with "personalities" as well as social roles, although there is a regrettable legacy by which African political actors are clearly identified while European administrators and missionaries are not. This will prove especially irritating to the student of Central African missions whose grassroots political activity contributes to the Mangwende case.

Distinctions between traditional and client chiefs have been found important in discerning varying relations between center and periphery, between bureaucratic efficiency and the abuse of patronage, between the

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prevalence of order and violence, and, while it is not placed within a wider theoretical context, Holleman's superbly detailed case study adds considerably to the quality of the body of data upon which such generalizations can be built. His thesis concerning the relativity of internal and external action (p. 22), while not carrying us much farther theoretically, provides the author with a consistent framework for the presentation of data at different levels. A commendable degree of attention is paid to statutory and historical material and to action at different levels within the hierarchy of power: imperial control and the Southern Rhodesian government; the government and the Native Department; the "Head Office" and the District Administration. Again it would be desirable to consider comparatively its application; Holleman's findings appear to contradict Low's suggestion that active local government participation may, as in Uganda, serve to postpone involvement in the national arena. It would seem that we are again driven to recognize a distinction between cultural pluralism in Africa in those societies which experience white settler domination and those which do not. And, having made the distinction, we are once more obliged to recognize our lack of a basic theory of group relations which would permit us to make full sense of both ideographic types.

JOAN VINCENT
Barnard College

From Union to Apartheid: A Trek to Isolation *by Margaret Ballinger. New York, Praeger Publishers, 1969.—499 pp. $12.50.*

Margaret Ballinger's *From Union to Apartheid* is a historical record of immense significance for those who are interested in what may arbitrarily be called modern South African history. It derives its importance firstly from the fact that Mrs. Ballinger was one of the gallant but, I think history will show, futile team of Native Representatives in the South African parliament who provided pinpricks for the South African white conscience. Secondly, as an historical record, the book is particularly useful in its presentation of materials and analyses of the Native Representative Council, that abortive experiment in African political representation which the African members of the N.R.C. themselves called a "toy telephone" through which they, as the elected representatives of the Africans, were supposed to communicate with the big white chief in the white parliament. Those who still think that Africans in South Africa are not complaining about apartheid ought to read the relevant portions of this book, and they will realize that it is not that the

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Africans are not complaining and rejecting the specious arguments put forward for apartheid, but that the world is just not listening to the Africans.

In spite of the fact that Mrs. Ballinger is a liberal, this book like its predecessors in the writing of African history, has a curious Eurocentric bias. Only the actions of the Europeans matter; when the Africans appear on the stage, they are qualifiers or extensions of white dominance, white policy, and white strategy. Even the title of the book focuses on the whites, for "Union" was essentially the union of white-man states and "apartheid" is a white ideological solution to what is regarded as a "Native problem." The statement, "Thus the history of the cape goes back over 300 years; the other provinces are something over 100 years old" (p. 17), must mean the history of the whites! Perhaps this is not accidental. She may very well share Trevor-Roper's view that "The history of the world, for the last five centuries, in so far as it has significance, has been European history"; and that the historians cannot afford to "amuse ourselves with the unrewarding gyrations of barbarous tribes in picturesque, but irrelevant corners of the globe."

Mrs. Ballinger makes a curious statement in her introduction (pp. 15-17). Discussing color and the racial ratios in South Africa as compared to Zambia, Kenya, and other countries in the north, she seems to imply that majority rule in South Africa is somehow inappropriate! Coming as it does from a professed liberal, the implication (if I read the passage correctly) is somewhat puzzling. The only black response to this would be: "Et tu Brute!"

ABSOLOM L. VILAKAZI
The American University

The Atlantic Slave Trade: A Census *by Philip D. Curtin. Madison, The University of Wisconsin Press, 1969.—xix, 338 pp. $7.50.*

Much good should be said about Philip Curtin's ambitious book. Quantitative studies of the Africans' westward migration across the Atlantic will be dated in the future, as is the case with Cortes's Mexico, before and after Curtin. He has not landed in a new world as the conqueror did, but he puts together so many disconnected pieces as to discover a novel and comprehensive image of the puzzle of slavery. Not much imagination is needed to foresee that scholars will hereafter spend page after page, not to give a new overall view, but to prove only that Curtin was wrong in particular details. He faces this fate, a noble one to be proud of. Let us play our part in that game.

Curtin calls his book a "census," a word which suggests a cross-sec-

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tion at a given time, rather than serialized estimates over four centuries from the early discoveries to the abolition of slavery. This is, perhaps, a petty reflection upon semantics, but there is a stronger disagreement to be voiced. Curtin looks at his topic in two dimensions: the distribution of the slave trade in space and through time. He deals in chapter 2 with the Hispanic trade, which includes a short report on the Portuguese business; in chapter 3, he deals with that of the North European colonies. Then he moves to develop his subject chronologically. Chapter 4 aims to review the fifteenth, sixteenth, and seventeenth centuries; chapters 5 to 7, the eighteenth; and chapter 8, the last one hundred years. Chapters 9 and 10, which close the volume, discuss trends and mortality en route to the Indies. Statisticians prefer the neutral, standard time divisions such as years, decades, and centuries. This book bows to that rule, but by bending to it the author seems to lose touch with history. Would it not be sensible to compare the figures with the evidence we have on the New World economy and society? According to the evidence, it is hard to believe that Spanish America imported during the second half of the seventeenth century, a rather deflated period, more slaves than during the first half, when wealth reached its peak there. Or, again, that there was no substantial increase during the prosperous first half of the eighteenth century, when trade became more regular and manpower needs grew. A division by rough meaningful periods should be preferred. Let me end by stating that as far as I know, Curtin's bibliography does not omit any important study.

NICOLÁS SÁNCHEZ-ALBORNOZ
New York University

Political Power in Latin America: Seven Confrontations *edited by Richard R. Fagen and Wayne A. Cornelius Jr. Englewood Cliffs, N.J., Prentice-Hall, Inc., 1970.—xix, 419 pp. $7.95.*

Barrios in Arms: Revolution in Santo Domingo *by José A. Moreno. Pittsburgh, University of Pittsburgh Press, 1970.—xiv, 226 pp. $8.95.*

Contemporary Latin American political life is characterized by violence or the prospect of a *golpe*. Chile, Santo Domingo, Mexico, and Venezuela are among the countries which have in recent times experienced presidential succession through the ballot box, albeit with widely varying degrees of freedom. Argentina and Brazil have undergone several transfers of executive power without participation of the electorate, and Castro has completed more than a decade as the supreme leader of the Cuban

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revolution. The two books under review contribute very substantially to an understanding of such developments and their significance by analyzing in some depth the round of struggles for control of power which took place in these countries during the mid-1960s.

Fagen and Cornelius have put together a reader distinctively different in type from the half-dozen which have appeared in recent years. Seeking a resolution of the dilemma of whether to study Latin American politics using the country-by-country or the topical approach, the editors have selected power crises as the unifying theme of their book so that "the reader with limited personal experience south of the Rio Bravo can appreciate the diversity of political styles in Latin America and at the same time sense the common factors or at least the common patterns that exist" (p. viii). Drawing in each case upon at least a dozen scholars and observers reflecting diverse viewpoints, they have succeeded admirably in their purpose, to direct "attention to such factors as political leadership, coalition building, and the impact of the international environment." The selections on "The Chilean Presidential Election of 1964" illustrate the orderly transfer of power through a highly polarized and emotional campaign in one of the region's most stable representative systems; those on "The Venezuelan Presidential Election of 1963" demonstrate a more difficult presidential succession in a violence-ridden situation. "The Argentine Elections and Coup of 1962" introduces the military as a decisive political actor, a theme which combines with other more ambiguous factors in "The Brazilian Coup of 1964." The role of the United States, secondary in the Brazilian case, becomes central to the section on "The Dominican Revolution of 1965." Students and the institutionalized political machine appear in "The Mexican University Strike of 1966," which is probably the least adequate of the cases. Senior partner Fagen's personal expertise on Cuba is abundantly evident in "Two Challenges to the Leadership of Fidel Castro." In a stimulating concluding essay the authors seek to link the substance of their cases to some of the basic propositions concerning Latin American politics current in this field. Throughout, the bibliographic references are more than adequate.

José Moreno, excerpts from whose earlier writings appear in the Fagen reader, provides the most original and insightful study yet published on this important question. A Cuban, ordained a Catholic priest, then subsequently trained in the United States and Europe, Moreno was conducting research in Santo Domingo on the eve of the 1965 revolution and civil war. Although he became an active participant, this study reflects his capacity as a qualified and systematic observer, highly conscious of the professional responsibilities thrust upon him by his unique opportunity to view from within the revolutionary movement from its

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inception. The internal dynamics of neighborhood paramilitary groups, relationships among leaders of the constitutionalist government, and the network of civilian support organizations are systematically explored. The major scholarly contribution is Dr. Moreno's sociological analysis of the composition of the rebel movement along with its members' shared attitudes, common aspirations, and emerging values. His typology helps facilitate comparison of the Dominican phenomenon with other rebel movements. In light of the lack of serious scholarship on the Dominican effort at revolution and the prevalent tendency in this country to play it down as just a minor episode, *Barrios in Arms* merits attention.

RONALD M. SCHNEIDER
Queens College, City University of New York

Crisis in Brazil *by Octavio Ianni, translated by Phyllis B. Eveleth. New York, Columbia University Press, 1970.—244 pp. $8.50.*

Octavio Ianni, formerly professor of sociology at the University of São Paulo, has written a controversial and provocative study of contemporary Brazil to explain in the broadest historical context the 1964 coup d'état. Brazil, according to the author, faces two alternatives: dependence and capitalism or independence and socialism. Professor Ianni emphasizes that the struggle to select between those two alternatives has has engendered the crises and tensions of contemporary Brazil. The first thrust toward popular democracy took place under the guidance of Getúlio Vargas, and over the decades since 1930 the conditions for a socialist revolution improved, particularly during the fifties and the early years of the sixties. Such a possibility frightened the agrarian, industrial, commercial, and financial bourgeoisie into uniting and then allying with the military to stage the coup of April 1, 1964. The military takeover inaugurated a "colonial-fascist regime" a dictatorship of the bourgeoisie. Brazil thereafter became more dependent than ever on the United States and on international capitalism.

The voice raised in this book echoes the sentiments and beliefs of Brazil's nationalists—and to a remarkable degree those of most Latin American nationalists. It is a voice little heard and even less understood in the United States. Our own scholars of the area generally fail to convey or to interpret that powerful emotion. After a recent visit to South America, an astonished Arnold Toynbee exclaimed, "Nationalism has, indeed, become a more potent religion than Christianity." Ianni gives his reader a clear statement of faith in that new "potent religion." Failure to understand its appeal and force, I fear, precludes an understand-

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ing of modern Brazil, or modern Latin America for that matter. The importance of *Crisis in Brazil* for a North American audience lies in its forceful statement of the nationalist cause. Unfortunately, an abrupt style and the assumption that the reader is well versed in Brazilian history limit the impact this book might have. A freer and more literary translation as well as an introduction and notes probably would have eliminated those difficulties. Still, any extra effort spent reading this book will be rewarded by the insight Professor Ianni gives into contemporary Brazil.

E. Bradford Burns
University of California at Los Angeles

Pressure Groups and Power Elites in Peruvian Politics *by Carlos A. Astiz. Ithaca, Cornell University Press, 1969.—xviii, 316 pp. $12.00.*

Peru *by Sir Robert Marett. New York, Frederick A. Praeger, 1969.—288 pp. $7.50.*

Professor Astiz has synthesized a wealth of material and provided us with one of the most comprehensive and coherent interpretations of contemporary Peruvian political development.

Relying on a class and sectoral analysis, Astiz examines the role of the upper, middle, and lower classes and their relationships to interest groups, parties, and institutional forces (church and military). Distinguishing between the various segments and substrata within each class, the author locates a "modernizing" middle class which is not tied to the traditional upper class. Yet it is not at all clear on what evidence the author bases his contention that this is a "modernizing" group—nor does he clearly indicate why it should be so.

In his discussion of the impact of development on the class structure, the author raises the problem of inequality, an issue frequently obscured by economists and social scientists overly preoccupied with "growth" and gross quantification: "The arithmetical improvements which various economic indicators have shown from time to time actually reflect improvements in the scale of living of the upper and middle classes" (p. 87). The powerlessness of the lower classes is translated into the lack of substantial socio-economic improvement. Astiz considers the ant-like incremental changes of the urban slums (the much vaunted "improvements" cited by Mangin and Turner) within a larger political and social context: the residents in the *barriadas* are the least improved, least influential, and most thoroughly exploited group in urban society.

Of particular interest is Astiz's discussion of the American Popular

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Revolutionary Alliance (APRA). In some detail Astiz outlines APRA's move to the Right and its opposition to democratic social reforms and nationalist measures. Astiz's discussion of APRA should be read by the APRA cultists of the "democratic Left" in the U.S. who have apparently allowed their partisan enthusiasm to blind them to contemporary realities.

Astiz's discussion of the church and military is balanced and judicious; it takes account of the sources of innovation as well as those of conservatism. In a postscript, however, the author tends overly to stress the conservative aspects of the current military junta, thus failing to anticipate the passage of the sweeping agrarian reform law.

Utilizing the notion of a "penetrated" society, Astiz focuses on the links between the domestic ruling class and U.S. economic interests and policy makers. While Astiz provides a useful conceptual framework for discussing the relationship of the "penetrators" (United States economic interests) and the Peruvian ruling class, he does not provide us with any details concerning the extent to which penetration has taken place, its impact on development (or underdevelopment), and the class structure. Nevertheless, in an exploratory way, Astiz has pointed to an important area in great need of further research.

Astiz concludes on a pessimistic note: that a revolution from below is as unlikely as is the possibility of a revolution from above. He tends to underestimate the current fissures in the military junta and the fluidity of the position within the military that could permit an opportunity for mobilization from below.

In contrast to the Astiz study, Sir Robert Marett's book is a breezy, somewhat superficial account of the development of Peruvian society. There is little that is original in the way of research and most of what passes for analysis is borrowed indiscriminately from a variety of conflicting sources with little or no attempt to synthesize the material. Apart from some of Sir Robert's quaint, ethnically-colored comparisons and his dismay over the decline of Britain's position in Peru, there are bits and pieces of information that could be useful if one could rely on the secondary sources from which they were culled.

JAMES PETRAS
Pennsylvania State University

Subversion and Social Change in Colombia *by Orlando Fals Borda, translated by Jacquelin D. Skiles. New York, Columbia University Press, 1969.—xiv, 238 pp. $6.95.*

This is an impressive effort at a dynamic political sociology with an

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historical dimension. Its specific theme is the process by which changes in fundamental social values have occurred in Colombia since the fifteenth century. Following Landauer and Mannheim, Fals Borda sees a dialectic between "topia" (stability and authority of an established order) and utopia (the assertion of a destabilizing new set of values). Whether the assertion of utopia leads to revolution or is frustrated, there occur some important changes which are incorporated in a new topia or synthesis. Through this dialectical approach, the author tries to handle the problem of asynchronous values, which many sociologists prefer to ignore.

On another level this is a political document. Fals Borda presents his analysis with the abstract terminology, and the charmless style, of the detached academic social scientist. But his political commitments are apparent. By employing historical analogy and sociological categories, he attempts to provide legitimacy for revolution, or at least social renewal, in contemporary Colombia. A key to this effort is his use of the term *subversion*. He strips this concept of the negative connotations generally associated with it, giving it the same normatively neutral, or even positive, application as the concept of innovation has had in economic history. *Subversion*, as Fals Borda defines it, is simply the assertion of new values (countervalues) against established ones by an emergent political group (counterelite). This objectification of subversion makes sense and is long overdue. It also permits Fals Borda to play a nice scholarly joke on his country's conservatives. If the late Father Camillo Torres (to whom the book is dedicated) was a "subversive," so, too, was the conquistador Gonzalo Jimenez de Quesada.

Fals Borda's analysis is in many ways admirable. Both the broad analytical framework and many of the specific points within it have much to recommend them. But the applicability of his categories to the data is sometimes obscure or questionable. In particular, readers not thoroughly familiar with Colombian history should be warned that his treatment of the political upheavals of 1848-54 (pp. 79-92) forces the facts to fit the symmetries of his analytical pattern. He asserts that the mobilization of Bogotá's artisans represented an attack upon the traditional acceptance of nature's domination. It is hard to see how one can apply this interpretation to the efforts of an already established economic group to defend previously existing protective trade policies. Similarly, he sees rural terror directed against landowners as an attack upon the traditional value of "otherworldliness . . . by way of the patterns of submission that prevailed in colonial days" (p. 80). Perhaps, but the point at the least needs elucidation. The author also sees the nineteenth-century liberals as the bearers of "mechanistic" values, when in fact con-

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servatives were the most ardent proponents of science and technology between 1820 and 1860. In this case Fals Borda falls into the common temptation to see "traditional" and "modern" values as synchronous and as consistently and unambiguously represented in two opposing socio-political groups. Finally, the author asserts that the alliance between young upper-class liberals and the artisans broke down because the liberal youth "ceded under pressures from their elders" (p. 86). This interpretation is erroneous and self-deceiving. Upper-class liberal youth were *never* in complete accord with the artisans. From the start, the upper-class youth promoted liberal ideologies, in particular free trade and economic laissez faire, while the artisans were militant proponents of tariff protection. The liberal youths, therefore, did not have to be pressured or "co-opted." Members of the landowning-mercantile aristocracy, their emergence as opponents of the artisans was class-determined. Thus the young liberals did not "take the side" of the Bogotá dandies; they *were* the dandies (p. 87). One wonders whether Fals Borda's misreading of co-optation in the nineteenth century represents simply a mistaken backward projection of current patterns. Possibly it also reflects self-deception about how co-optation occurs today.

FRANK SAFFORD
Northwestern University

American Boarding Schools: A Historical Study *by James McLachlan. New York, Charles Scribner's Sons, 1970.—x, 381 pp. $10.00.*

This is a book composed from materials in a Ph.D. dissertation. It has the merits associated with this sort of origin: it is excellently documented (there are 66 pages of notes and sources); it is rich in quotations from recondite sources (how many historians are likely to be familiar with the archives of Lawrenceville School, New Jersey?); and it achieves something novel, for no one has recorded the history of the American private school in the way Rudolph has recorded the history of the American college. Although this book lacks the sparkle and skill in selectivity of Rudolph's book, it is certainly worthy to stand on the same shelf; and it has an interesting story to tell.

In England, though not in Scotland and Wales, the private boarding schools (perversely labeled "public schools") have been exhaustively described in history and fiction and biography. There are 273 of them (this includes some nonboarding public schools), but they constitute less than 4 per cent of the secondary schools in England and Wales and they enroll only 1.4 per cent of the pupils. In public esteem scarcely a score

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of these 273 schools are ranked as "great" public schools—familiar names like Eton, Harrow, Rugby, Winchester, and a few more. The typical public school enrolls mainly boarders; in 1967 there were only 41,000 boys boarding at public schools out of a total enrollment of four million boys. Nevertheless, the English boys' public school system is known throughout the world. By contrast it comes as a surprise to many, otherwise enlightened, non-Americans that in the U.S.A., too, there are similar boarding schools, honestly labeled "private." It is these, Andover, Choate, Exeter, Groton, and a few more, which Dr. McLachlan has studied in depth.

The results of his study are interesting for any student of the history of education in democratic countries. It is part of his thesis that the American boarding schools were not modeled on the great public schools of England. I must confess that I am not quite persuaded by his arguments. It is true that the early American educators, including Jefferson, looked for inspiration to the continent of Europe. And it is arguable that the evident similarity between English and American boarding schools, and their comparable role in society (producing prime ministers from Harrow and presidents of the United States from Groton), may be a case of parallel evolution such as has produced the astonishing isomorphism between cacti in the Arizona desert and euphorbias in North Africa. But Groton, at any rate, owes its inspiration to an English public school, for its founder, Endicott Peabody, was a pupil at Cheltenham, and (as McLachlan vividly describes) Peabody deliberately modeled his school as a place to inculcate muscular Christianity in the English style.

The English public school system has recently been examined by a commission which has produced a report of conspicuous ambivalence. I would guess that a similar commission in America would find itself in a similar state of ambivalence. The fact is that many English parents still want to support private boarding schools, and indeed enroll their sons almost straight from the maternity hospital. At the same time there is a strong current of egalitarianism, which even Tory politicians cannot ignore, which is all for sweeping away these remnants (as they regard them) of class structure. After reading McLachlan's interesting chronicle, even a reviewer tinged with radical sentiments comes to the conclusion that there ought to be room, even in an egalitarian society, for Grotons and Etons. Not because they cater to the upper classes or confer prestige, but because they impart to their pupils a sense of responsibility to society.

ERIC ASHBY
Clare College, Cambridge

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The Rise of the Student Estate in Britain *by Eric Ashby and Mary Anderson. Cambridge, Mass., Harvard University Press, 1970. —x, 186 pp. $7.00.*

Until recently, a convention, observed on both sides of the Atlantic, dictated that historians approach the subject of higher education as either the history of a particular institution or the history of the academic profession. The result, with few exceptions, has been studies myopic in focus, partisan in tone, elitist and self-serving in conception. Not the least of the virtues of *The Rise of the Student Estate in Britain* is that its success in breaking with this hobbling convention should encourage emulation.

Sir Eric Ashby, Master of Clare College, Cambridge, and Mary Anderson (this is his eighth book on higher education and their third collaboration) begin their study of the influence students have had on British universities with an examination of the Scottish universities in the 1820s. From there they trace the development of a conscious student "estate" southward to the University of London and the provincial universities which, after 1900, encouraged the establishment of student representative councils. With the founding of the National Union of Students in 1921 the movement acquired an interuniversity dimension, and by 1965, as evidenced by joint meetings of the NUS and the Committee of University Vice-Chancellors, "partnership with the Establishment."

Citing examples of student influence from the reorganization of the Scottish universities in the 1830s to the 1963 Robbins Report, the authors effectively argue that such influence has been a constructive force in British university life. Indeed, their principal complaint is not with the students who have insisted upon being represented in the university decision-making process, but with those indifferent to that process. What must they think of American students who are just beginning to realize that such a process exists?

Only in the concluding chapter, where description gives way to prescription, does the authors' persuasiveness falter. Contending that the recent turbulence besetting British universities is unrelated to the earlier student movement—the work, rather, of an "unrepresentative" handful of revolutionaries mimicking Berkeley and Berlin—they propose a strategy of "aggressive tolerance" to deal with them. Disrupters ought to be suppressed, they argue, not by physical force, which gains them sympathy, but by the repeated intoning of the liberal tenets of civility and rationality and due process, which will isolate the revolutionaries from the mass of students and thus ultimately oblige them to vacate the premises.

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The authors' optimism is premised on their belief that the majority of students will be in the future as restrained and deferential as their representatives in the past and that they will continue to regard themselves as "a junior, inexperienced partner" in the work of reforming the university. Maybe so. Yet it might be argued that the traditional restraint was never a reflection of student commitment to the liberal tenets upon which the university's existence depends, as much as a reflection of student apathy and the absence of any in the university prepared to challenge these tenets. There is no such absence today, and however unrepresentative the challengers, the university may not survive them. And just when the historiography of higher education was coming into its own.

ROBERT MCCAUGHEY
Barnard College

The Sectarian College and the Public Purse: Fordham—A Case Study *by Walter Gellhorn and R. Kent Greenawalt. Dobbs Ferry, N.Y., Oceana Publications, Inc., 1970.—xi, 212 pp. $7.50.*

The two law professors who did this case study ask and answer the question: what kind of alterations would Fordham University have to undertake in order to be considered a nonsectarian institution of higher learning under the laws of New York State? To put it more bluntly, they were concerned about the changes that might be needed to establish eligibility for state financial subvention. They are also ready to suggest that public aid is not worth having if it can be gained only by ignoring the goals a college or university is striving to attain.

This was the dilemma faced by Fordham University in the academic year 1967-68 when this study was made. Only four years earlier in 1963 in the report of its own self-study, Fordham had declared itself committed to the traditional religious and Jesuit objectives in higher education. Meanwhile, one financial crisis followed another and the problem became one of survival. The epilogue in this book (pp. 117-29) shows that Fordham accepted the recommendations of the lawyer-researchers. The university made itself eligible to dip into the public purse, so that by February 1970 the commissioner of the New York State Education Department could conclude that Fordham was no longer a sectarian institution.

Other church-related schools had faced the same situation. Like good lawyers, the authors cite pertinent Catholic precedents without looking into the earlier history of Protestant colleges. Various elements in the shift to secularization and laicization had already appeared at George-

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town University and St. Louis University (both conducted by the Jesuits), and especially at Alverno College and Webster College (both run by religious sisters.) The main requirements of the shift were to remove control by the church people and to remove the religious purpose of educational content.

Regardless of what has happened (or will happen) elsewhere, the Fordham case represents a thoroughly researched model for church-related colleges that need to reform themselves sufficiently to obtain public funds. The authors discuss the fact that federal legislation on separation of church and state is uniformly applicable everywhere in the country, but state constitutional provisions vary widely. They provide a valuable appendix listing these constitutional qualifications state by state.

The central problem of survival for private colleges and universities is financial, but not every state promises subvention for private institutions. There is everywhere, however, a general trend to secularization, and the church schools are following the trend. More subtle, perhaps, is the trend to declericalization within the Roman Catholic church, which means that whatever the financial problems, its educational system moves ever closer to laicization.

JOSEPH H. FICHTER
Southern Methodist University

The Social Bond: An Introduction to the Study of Society *by Robert A. Nisbet. New York, Alfred A. Knopf, 1970.—xix, 425 pp. $10.95.*

Rarely does one come across a book intended to introduce students to the bare bones of a discipline that is a pleasure to read. This is such a book. Written with elegance, Nisbet's work provides a lucid and economical exposition of most, though perhaps not all, of the major ideas and issues that are at the center of present-day sociological inquiry.

As the title implies, Nisbet is mainly concerned with the problems of social order, with the bonds that tie individuals to their society and link them to their fellows. He acquaints the students with the major concepts on the nature and functions of these social bonds, be they the fruits of sociological studies in the past or of more recent vintage, and thus enables the reader to assess the "relevance" of sociology for an understanding of our current predicaments.

The central part of the book deals with the major conceptualizations which sociologists have found useful in carving out of the bewildering concreteness of social reality those abstracted structures and processes

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which further the disciplined understanding of social order. We are treated to lean and compressed discussions of, *inter alia*, social interaction, social authority, social roles, social status, and social norms. Alienation, anomie, and deviance are discussed under the heading "social entropy," a term borrowed from physics where it suggests a running down of available energy. I doubt that this is a useful term in sociology. Deviance, for example, may upon occasion be the harbinger of a restructuring of society, a better harnessing of energy, rather than a sign of decay.

The last part of the book, which is devoted to social change, seems to me less satisfactory than the rest. Here Nisbet once again provides us with Teggartian variations on the theme of the unpredictability of the course of human history. He rightly has his sport with simplistic theories of unilinear evolution or progress, but he seems to be on the wrong track when he asserts dogmatically that "to seek to derive change . . . from the elements of social structure . . . is . . . fallacious and self-defeating. . . ." If that were really the case, much of the sociological enterprise would become nugatory. It seems certain, for example, that changing birth rates are related to changing family structures and changing authority relations, and it is more than plausible that the change from rural to urban and suburban residence patterns in the United States has structural causes. Teggart's attention to event, time, place, and circumstance in the realm of historical explanation can be useful for explaining the unique constellations of historical events. But sociologists would relinquish their birthright were they therefore to give up the quest for underlying patterns or structural uniformities.

This is a book for beginners, yet it never talks down to the reader. It seeks to involve him in a dialogue by stimulating his sociological imagination so that he can ask questions that really matter. I hope that it will find many readers.

LEWIS A. COSER
State University of New York, Stony Brook

Race and the Social Sciences *edited by Irwin Katz and Patricia Gurin. New York, Basic Books, Inc., 1969.—xii, 387 pp. $8.95.*

This book contains papers by eight well intentioned and cautious social scientists. One of the editors, Katz, is known for a series of important studies demonstrating variations in performance among groups of Negroes under different experimental conditions. The papers present potential research strategies for the investigation of race relations, exclusive-

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ly among whites and blacks, and useful data on such substantive issues as education (Pettigrew), population and housing (Taeuber), jobs and income (Killingsworth), and race relations and social change (Coleman). The final chapter summarizes the contribution of each author and brings forth a series of assumptions and suggestions concerning the determinants of performance and the bases of prejudice and discrimination.

The book has a rather flat tone, which will please those who demand total objectivity in social science research. In this case, however, the objectivity is restricting. The approach is modern in the methodological sense, but much important social content is passed over almost as if it did not exist. There is little effort to come to grips with some of the major changes in black society: the turn away from integration toward black power, the militant movements such as the Black Panthers and other radical groups, and the interesting experiment in social relations carried out in Chicago by blacks and southern white migrants which resulted in the formation of the Young Patriots. Nor is any space given to the practical problem of dealing with a national administration that has withdrawn funds from research and social services and turned away from urban minorities in general and blacks in particular.

The "culture of poverty" appears as a working principle, although many anthropologists see it as a cover for the effects of society as a whole on its poor. The Moynihan Report is accepted by most of the authors, although questioned by Taeuber. In sum, while the authors acknowledge the shortcomings of white society in relation to certain areas of social structure, some tend to put the cultural burden for change on the black community.

With all its shortcomings this is an important work which deserves attention. The data presented are of great importance for the administration of practical programs in the area of race relations.

ALEXANDER ALLAND JR.
Columbia University

Structure and Performance of the U.S. Communications Industry: Government Regulation and Company Planning *by Kurt Borchardt. Cambridge, Mass., Graduate School of Business Administration, Harvard University, 1970.—viii, 180 pp. $6.00.*

This book is an original, thought-provoking contribution to the political economy of regulation. Borchardt sets himself an ambitious threefold task: to examine the "organizations" which participate in regulatory decision-making in the communications industry; to analyze the

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"reasoned arguments" they employ in vying to persuade regulators to change telecommunications structure in particular ways; and to delineate the decision-making procedures wherein these arguments are articulated and weighed. Borchardt proposes in short to give us a relevant conceptual framework in which to study and evaluate the complex of decisions which produce structure and structural change.

Addressed mainly to the participating business entities (though also to the government administrators), the book makes explicit much of the regulatory process to which they are all parties. That process is one of adjustment, bargaining, negotiation, and compromise, wherein industry goals and government priorities should ideally interact within procedures and safeguards which produce agreement, not polarization. For Borchardt, the range of policy options can be set as much by a forward-looking industry establishment as by government initiatives, and the final bargains will be struck within that range. Regulation is conceived in the broadest executive, legislative, judicial, and administrative terms, and interindustry negotiation may even remove certain bones of contention from the regulatory arena entirely.

Economists are clearly not the principal audience for this book. The author makes explicit from the outset his conviction that optimal cost/benefit solutions cannot be readily identified let alone "imposed from above." Yet Borchardt's failure to distinguish his concepts more fully from those of conventional industry studies may limit the deserved attention his book receives among professional economists.

For Borchardt, structural regulation provides public resolution of such questions as these: Shall private or specialized carriers be authorized to compete with common carriers? Shall a single joint venture (including but not limited to common carriers) operate a sole international or domestic satellite system, or shall the government, or the common carriers alone, be authorized to do so? Shall private specialized satellite systems also be licensed? Shall local UHF service be nurtured by braking CATV growth, or shall CATV be permitted to provide a nationwide subscriber-supported competitive alternative to advertiser-sponsored off-air national networking? However, the author makes little attempt to establish clear-cut links between these alternative structures and the business conduct and social-economic performance that would result. He is more interested in how telecommunications structure emerges and changes than in what happens *within* it at any time. This makes an interesting contrast with more conventional economic studies, where regulation's impact is often viewed as affecting mainly pricing behavior, investment decisions, and conduct patterns generally, not structure.

One final problem is the book's failure to analyze the end effects of

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alternative regulatory options. It comes closest to this in the main chapter, IV, where the author draws attention to trade-offs between divergent industry and government priorities. But one important contribution of economic analysis may have been inadvertently slighted at that point. Quantitative economic knowledge of the short- and long-term effects of regulation is obviously of great interest to both industry and government participants. Injection of such knowledge back into the decisional arena could conceivably bring the performance of Borchardt's interacting system closer to those very norms which regulatory economists work with increasingly today—product quality, reliability, safety, diversity, wide geographic availability, nondiscriminatory access, etc. Tested economic fact presumably has its place along with other inputs (from other disciplines) in the "reasoned arguments" Borchardt's participating organizations will use in their attempts to alter structure. But the political viability of industry and governmental initiatives will clearly vary with changing public knowledge of their end effects.

HARVEY J. LEVIN
Hofstra University

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Book Notes

The appearance in 1936 of J. M. Keynes's *General Theory of Employment, Interest and Money* and its rapid and profound impact on Western economics could not possibly go unchallenged by the Soviets, for its message was that the very phenomenon that in the thirties seemed to doom capitalism to extinction, its disastrous instability, was after all not theoretically inevitable and could be averted by proper policy. Even more disturbing to the Soviets was the espousal of more-or-less Keynesian policies by major Western governments and the prosperity of the West after the war. But under the conditions of Stalinist terror, the overt Soviet response to this challenge could be little but an outpouring of dogma-rooted invective, occasionally livened up for the reader by academic billingsgate. It is this unedifying record that is fully cataloged and liberally quoted in Carl B. Turner's *An Analysis of Soviet Views on John Maynard Keynes* (Durham, Duke University Press, 1969; vii, 183 pp., $6.50). The author's search was thorough. The book also brings out the ambivalent attitude toward Keynes (the critic of the Versailles Treaty versus the hater of bolshevism) in the early years of the regime; the toning down of the invective and the somewhat greater subtlety of understanding of Keynesianism following the initial de-Stalinization by Khrushchev; and the interesting fact that the Soviet translation of the *General Theory* in 1948 was an expurgated one (though, in true style, the reader was given no warning of the deliberate omissions). There is little more to the book than this. While the author could have chosen a broader frame of reference (and a more felicitous style), it is unfortunately the case that a definite analysis of Soviet reactions to Keynesianism and the postwar performance of Western economies cannot be done until the relevant archives are opened to impartial scholarship. When the book is written it will make fascinating

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reading, and not for economists alone.—GREGORY GROSSMAN, *University of California, Berkeley.*

The demand for local control of municipal services and institutions receives sympathetic treatment in *Neighborhood Government: The Local Foundations of Political Life* (Indianapolis, The Bobbs-Merrill Company, 1969; xiii, 111 pp. $2.95) by Milton Kotler. This slender volume posits a romantic view of community independence and indicts annexation as the primary cause of neighborhood decay. According to Kotler, imperialistic large cities used annexation, particularly in the nineteenth century, to deprive nearby villages of their autonomous government. The result was the exploitation of the then subservient neighborhoods by downtown interests and the inevitable decline of the formerly proud villages. To restore local prosperity, the author draws upon his experience with the East Central Citizens Organization of Columbus, Ohio, and suggests the creation of community corporations. Ultimately, these neighborhood governments should develop semi-independent economies and gain political control of finance, imports and exports, war and peace, territorial defense, and laws. Unfortunately, Kotler not only ignores political realities but offers no support for such assumptions as his belief that neighborhoods usually pay more to the city in taxes than they recover in services. He asserts that the impetus for urban growth is a function of neighborhoods rather than the city as a whole, which may be true, but it does not follow that peripheral growth occurs independently of the core city. He views neighborhood development and decay as a political process and understates the importance of demographic trends that often occur without regard to the political status of an area. More important, the author's research on annexation history is superficial. As a result, his facts are sometimes erroneous and his examples atypical. The movement for neighborhood independence must continue to await its historical justification.—KENNETH T. JACKSON, *Columbia University.*

The 1967 *Yearbook of the United Nations* (New York, Office of Public Information, United Nations, 1969; xi, 1110 pp. $25.00) maintains the high standard the editors have hitherto set. Generally, it follows the same topical arrangement as its recent predecessors. The fact that it is over 100 pages shorter than the 1966 volume is explained by the inclusion in the latter volume of the text of the judgment of the International Court of Justice in the South West Africa case. The 1967 volume suffers from no comparable distortion and the space given to the various

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topics reflects with considerable accuracy their importance in the activity of the organization. In the light of developments in the Middle East, it is not surprising that political and security questions require more space than in the previous volume. On the other hand, the considerably greater space given to questions relating to trusteeship and non-self-governing territories does appear to be a concession to the special interests of the new nations. The documentary references are well done and are of great value to those who wish to pursue these matters further. —LELAND M. GOODRICH, *Columbia University.*

The Structure of Political Geography edited by Roger E. Kasperson and Julian V. Minghi (Chicago, Aldine Publishing Company, 1969; xii, 527 pp. $12.50) is a collection of readings intended as a textbook, not a methodological treatise as the title may imply. The content and the organization reflect dominant educational trends of the 1960s—an academic discipline and its theoretical structure. To the basic question of whether or not a collection of essays by numerous authors achieves a definition of the structure of political geography, this reviewer would reply that the editors have made the best attempt to date but still fall short of achievement. The essays in general reveal the diversity of studies done by political geographers, highlight the lack of a uniform methodology or research focus, and hence emphasize the absence of a singular structure. The editorial comments provide cogent and valuable comments on political geography, but fail to provide a structural definition of the field which would explain the rationale for the inclusion of several items. Since no compiler of a collection of this type would necessarily select the same essays, it is perhaps redundant to remark on the specific essays chosen by Kasperson and Minghi. But two observations are in order. About one-third of the essays are included in other readers or reprint series, a situation which indicates that the book encompasses major concerns of political geography, with the exception of regional problems. Some selections, however, are so related to the particular viewpoints of the compilers that the reviewer can but wonder how they are related to political geography. The absence of editorial definition is all the more regretted. The appendix (Aids to Research in Political Geography) is a valuable innovation in textbook content, particularly as a bibliography for students. In sum, as a textbook organized around methodological issues which are illustrated by essays of various authorship rather than focused on regional problems, this book is better than its competitors. As a statement of the structure of political geography, it adds nothing beyond the ideas of the essays which it reproduces.—DOUGLAS R. MCMANIS, *Teachers College, Columbia University.*

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